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DISSERTATION

Presented to the Faculty of the Graduate School of
The University of Texas at Austin
in Partial Fulfillment
of the Requirements
for the Degree of

DOCTOR OF PHILOSOPHY

THE UNIVERSITY OF TEXAS AT AUSTIN

May 2021

The Consequences of Political Persuasion in Greater China

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The University of Texas at Austin, 2021

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When and how does political persuasion employed by authoritarian regimes influence the attitudes and behavior of domestic and foreign audiences? Focusing on China, this dissertation comprises three essays in which the effectiveness of political persuasion in public education and mass media was examined. The overarching finding is that the Chinese regime's persuasive efforts are more effective among citizens who have familial connections to state patronage, who are predisposed to be regime-friendly, and who hold relatively weak political positions. By contrast, China-related persuasion often has a limited effect, sometimes even backfiring, for people who lack these background characteristics. A wide range of methodological approaches was adopted in this dissertation, including randomized controlled trials, natural experiments, survey research, and qualitative interviews. The findings reveal the potential and limits of the regime's propaganda and indoctrination, showing how persuasion works and among which segment of the population it works best. Despite devoting considerable resources to projecting influence both domestically and abroad, the Chinese government appears to win the hearts and minds of targets only under certain conditions.

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Chapter 1

Introduction

When and how does China-related political persuasion influence the attitudes, opinions, and behavior of both domestic and foreign citizens? Political leaders in China have long been mindful of creating favorable public opinion and a positive public image. In China, virtually every conceivable medium that produces, transmits, and conveys information to the public falls under the country's propaganda system.¹ Outside its borders the Chinese government in recent years has annually invested as much as 1.3 billion US dollars in increasing the global presence of Chinese media (Reporters Without Borders, 2019). The government also buys space in foreign media outlets to publish news articles and advertisements that are camouflaged as standard news stories (Dai and Luqiu, 2020).

The empirical evidence on the effectiveness of China's persuasive efforts is mixed: some scholars find that exposure to proregime messages sways people in the intended direction, but others show that the impact of propaganda is negligible and even counterproductive. In addition, little well-identified evidence tells us whether China's media influence operations are effective. Together, the current state of the literature indicates that the key question is not merely whether China-related persuasion can successfully shape political attitudes and behavior but also when and how it can do so.

Moreover, studying the effect of political persuasion on individuals is methodically challenging because people's information consumption is highly selective. People who choose

¹As such, propaganda does not carry negative connotations for the Chinese government and most citizens. The scope of propaganda oversight includes news and media departments (e.g., newspaper offices, radio stations, television stations, and magazines), educational organs (e.g., universities, middle schools, primary schools, vocational or specialized education, and cadre training), cultural organs (e.g., film/drama theaters, clubs, film production studios, and musical/theatrical troupes), and cultural facilities and commemoration exhibition facilities (Shambaugh, 2007, pp. 27-28).

to tune in to certain persuasive messages may differ systematically in ways that matter to their political attitudes and behavior from those who do not. This endogeneity issue vitiates the ability to disentangle cause from effect. The three essays in this dissertation are a collection of my efforts to address the empirical challenges as well.

The central argument in this dissertation is that the impact of China-related persuasion is conditional on the message recipients' predispositions. In contrast to previous scholarship that adopted a hypodermic needle model in which individuals are treated as passive recipients of persuasive messages, I emphasize the role of human agency in processing and reacting to information received. In other words, most people (even those living in authoritarian countries like China) are not passive but active audiences. My core finding in this dissertation is that China-related persuasive messages are more effective when they tap into people's political predispositions and reach people whose predispositions are relatively weak. By contrast, persuasive messages tend to have a limited effect, even backfiring, for people who are dismissive of the Chinese regime *ex ante* and those who find the messages incompatible with their actual experiences. In sum, despite devoting considerable resources to projecting influence both domestically and abroad, the Chinese government could win the hearts and minds of targets only under certain conditions. My use of a wide range of issue domains and methodological approaches helps generate a robust empirical foundation for this conclusion.

In Chapter 2, I focus on China's political education and its influences on students. Formal education is a central tool for government-sponsored indoctrination. Political elites choose and adjust educational content to fulfill their goals, such as promoting state-sanctioned ideologies and compliance with political rule. How effective is this state indoctrination? I conduct a quasiexperimental analysis exploiting the sharp variation in textbook content generated by China's most recent curriculum reform to examine the causal effect of politics textbooks on students' political attitudes. Building upon research on motivated reasoning and family socialization, I argue that only those individuals whose parents have connec-

tions to political patronage are subject to state indoctrination because their proregime biases transmitted from parents induce higher receptivity *ex ante* to government messages. Results based on a national survey show that the new politics textbooks successfully affected only those individuals whose parents had worked for the government. The findings not only highlight the role of intergenerational transmission in moderating the effectiveness of state indoctrination but also cast doubt on the actual degree to which regimes can change minds by changing educational content.

In Chapter 3, I examine the electoral effect of a major pro-Beijing media outlet on voters. One common approach to launching overseas information campaigns is the cooptation of foreign media outlets to disseminate preferred messages. I use a field experiment during the 2020 Taiwanese presidential election to examine whether and how a major pro-Beijing media outlet influenced individuals' vote choices and opinions about China. Weeks before the election, I randomly provided voters with real-time political news articles from the pro-Beijing media outlet on a website and tracked their exposure patterns using web traffic data. Results based on a panel survey at the individual level show that pro-Beijing news nudged people exposed to it to vote for China's preferred candidate and adopt more positive attitudes toward China. Yet the pro-Beijing media outlet in this study had a negligible effect, sometimes even backfiring, for voters who had been dismissive of China *ex ante* and those who think this media outlet is affiliated with the Chinese government.

In Chapter 4, I investigate whether Chinese business elites increase tax compliance when they are prompted by persuasive information about a recent expansion of government responsiveness in local China. Results based on a survey experiment of prospective business elites in China show that the information has a negative impact on people's willingness to pay taxes. My interviews suggest that the backlash seems driven by people's views of the information as the government's propaganda message contradicting their beliefs. The findings not only offer evidence of the fiscal social contract in an authoritarian context but also demonstrate a case in which people negatively react to proregime messages arguably used

to increase citizen compliance.

The findings of this dissertation yield several important implications. First, the empirical results of the conditional persuasion suggests that political leaders in authoritarian countries like China invest resources in propaganda and state indoctrination may aim to reinforce people who are already existing believers of the regime rather than expanding the popular base of the regime. Second, the evidence on the backfire effect contributes to the literature on the potential backlash effect of exposure to dissimilar political information. Some new studies involving the case of the United States indicate that political persuasion rarely backfires even under the most desirable conditions (Guess and Coppock, 2018; Wood and Porter, 2019), but this dissertation shows that the boomerang effect is not elusive. Future work can identify the conditions under which the unintended consequences of an attempt to persuade are more likely to occur.

Chapter 2

Family Matters: Education and the Conditional Effect of State Indoctrination in China

2.1 Introduction

Formal education is a central tool for government-sponsored indoctrination. In both democracies and autocracies, political elites choose and adjust educational content to fulfill their political goals, such as promoting state-sanctioned ideologies and compliance with authoritarian rule (Cantoni et al., 2017; Lott, 1999; Testa, 2018; Voigtländer and Voth, 2015), molding national identity and citizenship (Darden and Grzymala-Busse, 2006; Huang, 2019a; Naval, Print and Veldhuis, 2002; Nozaki, 2008), stirring up patriotism during political crises (Ben-Porath, 2007; Rosen, 1993), and teaching the ideas of civil liberty, procedural fairness, and voting (Niemi and Junn, 1998).

How effective is this state indoctrination? A study by Cantoni et al. (2017) uses China's most recent high school curriculum reform to examine the causal effect of a set of new politics textbooks on students' attitudes. The reform has brought notable variation in the content of political education offered to students; more importantly, the new curriculum was introduced to provinces in different years, helping researchers to account for unobservable cross-cohort and cross-province differences that may otherwise confound the impact of the curriculum change. Cantoni and his colleagues concluded that the new politics textbooks successfully changed students' political attitudes in the direction intended by the Chinese government.

Despite their prominence in the field, evidence presented by Cantoni et al. (2017) is based on students from China's most prestigious university, Peking University, which naturally raises the question of whether their results can be generalized to the effects of the

new textbooks on other Chinese students exposed to them. I use a national survey with a more representative sample to answer this question. A wide variety of respondents in the national survey also enable me to evaluate which segment of the population on which propaganda works best. In addition, most students surveyed in their study were just out of high school, but respondents in this study have mainly graduated from universities. I thus have much leverage to examine whether the treatment effect (if any) is sticky over time. In sum, this paper provides a critical test of Cantoni et al. (2017).

I argue that the receptivity of individuals to government-sponsored indoctrination is conditional on their familial connections to state patronage. Scholars of information processing have claimed that people's susceptibility to persuasion depends on whether the messages tap into their predispositions (Kunda, 1990). Political socialization scholarship has also established that parents play a pivotal role in shaping the political predispositions of their children (Hyman, 1959). When parents are connected to state patronage, the child is more likely attached to the regime and possesses higher receptivity *ex ante* to government messages. More generally, family socialization could affect differential responses of young people to political propaganda.

I employ a generalized difference-in-differences design that leverages provincial variation in the timing of curriculum reform and cohort variation in new curriculum eligibility. I restrict attention to individuals who started high school around the curriculum reform years. Because these people straddled the period in which the reform was implemented, they were differentially exposed to the textbook content depending on school entry years. I compare attitudes targeted in the new politics textbooks of those who were just young enough to study the new textbooks and those who were just too old to study them. Under the assumption that the group characteristics are effectively identical, disparity in targeted attitudes across the two groups could be attributable to the curriculum change.

Based on changes in textbook content and data availability, I examine three main politi-

cal attitudes that the government wished to shape.¹ Results show that the new textbooks affected only those whose parents had worked for the government. Among the government-affiliated students (hereafter affiliated students), those learning from the new textbooks are more inclined than those learning from the old ones to (1) support government intervention in citizen life, (2) accept socialist democracy, and (3) trust government officials. By contrast, the new textbooks had no demonstrable effect on those whose parents are not government employees — the great majority of the population. Indeed, when using the sample where both affiliated and nonaffiliated students are pooled together, the effects are nearly null. My findings are robust to a wide range of additional analyses. Two falsification tests further confirm the identification of the treatment effect. I also discuss four main alternative explanations of the results in this paper.

For the majority of the respondents whose parents are unaffiliated, the textbook change made no difference whatsoever. My finding thus contrasts with the conclusion of Cantoni et al. (2017). Why do the two studies yield different results? One possibility is that Peking University may overrepresent affiliated students because it is the most prestigious university in the country. Indeed, 54% of the Cantoni sample had parents in the Chinese Communist Party. A recent survey from Peking University also shows that *cadre* in party-government organs and public institutions has become the most common occupation among the students' parents since 1997 (Liang and Lee, 2012). Add in students whose parents are connected to state patronage and the Cantoni sample becomes something of a complement to the national survey used in this paper. In the concluding section, I discuss two other possible reasons that may explain the different results, including different years of reform covered and effect duration.

¹The changed textbook content will be discussed in a later section. Admittedly, the new textbooks contain more new topics than what I study, but because the national survey used in this paper was not designed to assess the impact of the curriculum change, the variables pertaining to the new textbooks are under constraint.

2.2 When and How is Government Propaganda Effective?

2.2.1 Motivated Political Reasoning

Individuals' political priors matter with regard to receptivity to persuasion. People process and examine new messages in a biased manner to uphold their priors (Ditto and F. Lopez, 1992; Kunda, 1990; Lord, Ross and Lepper, 1979; Nickerson, 1998; Taber and Lodge, 2006). They are less skeptical consumers of a message consistent with their priors: they use limited cognitive efforts to evaluate the validity of the message, judge it as relevant and reliable, give undue weight to evidence in the message that supports their expectations. By contrast, people allocate more cognitive resources to thinking about a challenging message, focus on its weaknesses, and scrutinize its argument hypercritically.² The existence of motivated reasoning suggests that state propaganda may exert its intended effects only among people with proregime biases but fails to do so among those without adequate biases.

Although the argument that propaganda is effective among people with higher receptivity *ex ante* is not new, individuals' political predispositions in most prior studies are crudely inferred from macrolevel factors, such as political regimes and resident districts.³ For instance, some studies claim that people's past socialization under a certain political regime nurtures their priors against a new regime's propaganda (Bleck and Michelitch, 2017; Geddes and Zaller, 1989). Other studies show that people born in districts where anti-Semitism was historically high were particularly susceptible to Nazi indoctrination because of their existing prejudices (Adena et al., 2015; Voigtländer and Voth, 2015). In contrast to these studies, I focus on a microlevel factor — individuals' family socialization — that better captures the nature of people's political predispositions.

²This does not mean that people never accept persuasion that challenges their priors, but because people react to the messages with excessive skepticism, the messages require stronger and more unanticipated evidence than necessary to induce people to believe them (Chiang and Knight, 2011; Huang, 2015b).

³Peisakhin and Rozenas (2018) is one exception.

2.2.2 Intergenerational Transmission of Political Predispositions

Families and parents are commonly viewed as the "foremost among agencies of socialization into politics" (Hyman, 1959, p.69). Parents transmit attitudes that they consider valuable for their children to hold, presenting examples or models that children may emulate (Hess and Torney-Purta, 1967). When parents have a close link to the polity (in the form of public employment for instance), they more enthusiastically promulgate values that support political authority (Merelman, 1980). In addition to value transmission, parents situate their children in a sociopolitical environment where the latter develop attitudes as a result of the life experiences that accompany the inherited environment. The shared environment facilitates parent-child attitudinal similarity (Hout, 1984). As far as utility maximization is concerned, people think like their parents politically because they expect to have experiences with the regime similar to those of their parents (Achen, 2002).

Parental transmission of political predispositions is a staple in the field of political socialization. Although many studies have shown that parent-child correspondence on political values is more limited than socialization researchers expected, they mostly agree that children's political attachments are highly congruent with those of their parents (Alford, Funk and Hibbing, 2005; Jennings and Niemi, 1968; Jennings, Stoker and Bowers, 2009; Niemi and Jennings, 1991).

2.2.3 Cooptation in the Form of Political Patronage

What kinds of parents are likely to hold proregime biases? A large body of literature has shown that elites in developing and authoritarian states can exploit public employment via such methods as controls over recruitment, promotion, and retirement of government posts to create stakeholders in the status quo (Blaydes, 2010; Kitschelt and Wilkinson, 2007; Greene, 2007; Svolik, 2012). Public employment is a main channel through which governments allocate state patronage to garner popular support (Calvo and Murillo, 2004; Gimpelson and Treisman, 2002; Remmer, 2007).

The Chinese Communist Party (CCP) uses the *bianzhi* system to control the amount of official employment in managing the scope of state patronage (Burns, 2003, p.777); it comprises all positions officially created. Following previous work, I define government employees as personnel serving in the Party, the government, and public institutions (Ang, 2012).⁴ In China, working units in the Party and governmental organs can be divided into core bureaus (*jiguan danwei*) and public institutions (*shiye danwei*). Core bureaus, which are responsible for political, administrative, and regulatory work, have a cluster of public institutions that perform such delegated tasks as administration, provision of public services, and commercial activities.

In sum, I hypothesize that government-sponsored indoctrination influences only those students whose parents are government employees because they are predisposed to accept government messages as a result of intergenerational transmission. Before testing this hypothesis with a rigorous research design, I show supporting evidence to two observable implications of my argument. The first observable implication is that government employees should possess more proregime attitudes than nonemployees. Using data from the China Survey 2008, Appendix Section A presents the evidence that government employees are significantly more inclined than nonemployees to trust government officials, feel satisfied with government performance, and feel pride in being Chinese, all things being equal. The other observable implication is that affiliated students should have stronger political attachment to the Party than nonaffiliated students. Focusing on CGSS respondents who started high school around the curriculum reform years, results reported in Appendix Section A show that affiliated students are significantly more likely than nonaffiliated students to submit membership applications to the CCP, viewed as a display of proregime bias.

⁴This definition excludes personnel in the military and state-owned enterprises, a practice commonly adopted in existing studies because they are managed differently from public bureaucracies.

2.3 Empirical Strategy

To formally evaluate my hypothesis, I use variation in the content of political education generated by China's most recent high school curriculum reform. I identify the causal effects of political education in high school by comparing attitudes targeted in the new politics textbooks of those who were just young enough to study the new textbooks and those who were just too old to study them. One has no reason to suspect a substantial difference in personal characteristics in these students after accounting for common characteristics of province of origin and cohort. Thus, discontinuity in targeted attitudes between students studying the old and new textbooks is likely caused by state indoctrination efforts.

2.3.1 Political Education in High Schools in China

China's political education in high school is a canonical example of government-sponsored indoctrination, where courses aim to "help students recognize correct values and grasp correct political direction."⁵ It is part of thought work in schools, aiming to shape the political and social beliefs of students to promote their faith in the CCP leadership and socialist system. Under the current Chinese educational system, high school students are required to complete four political education courses in their first two years of high school, spending two hours a week taking these courses. The four courses are Economic Life, Political Life, Cultural Life, and Philosophy, each taught with a textbook bearing the name of the course as its title and focusing on one specific topic. The order in which students register for the four courses is not nationally uniform but depends on student choice or school circumstances. I refer to these four textbooks as politics textbooks throughout the paper.

⁵This quotation is a translated excerpt from a government document: "Curriculum Framework for the Senior High School Politics Subject." <https://tinyurl.com/y6tcsa5n>.

2.3.2 The Eighth Curriculum Reform in China

The Eighth Curriculum Reform, the most recent one, was officially initiated after the Ministry of Education issued its "Outline of Basic Education Curriculum Reform" in 2001. According to this document, the primary goal of the reform is to facilitate the moral and ideological education necessary in the current political, economic climate.⁶ The reform was described by government officials as "historically important" and one of the most significant changes in educational policy since China's economic reform in the late 1970s.

Between 2004 and 2010, the government implemented the curriculum reform, bringing substantial changes in textbook content. Three features make the curriculum reform an appealing case to study the impact of state indoctrination. First, the initial cohort of students studying the new curriculum would have an entirely different three-year curriculum from those who started high school just a year earlier. This also means that the older, prereform cohorts of students would not switch to study the new textbooks. This reform thus generates sharp variation in educational content offered to students who started high school around the reform years.

Second, in contrast to educational reforms occurring at a "critical juncture," such as regime change (e.g., from Weimar to Nazi Germany) and political crisis (e.g., after the Tiananmen Incident), which often coincides with other socioeconomic changes that may confound the effect of curriculum reform, the Eighth Curriculum Reform was not followed by any major political changes in China. As a result the variation in educational content used in the analysis enables me to isolate the impact of political education more cleanly. Third, the Chinese government introduced the new curriculum to provinces during different years.⁷ This incremental approach creates two types of cross-sectional variations — cross-cohort variation within provinces and cross-province variation within cohorts — that

⁶Available at http://www.gov.cn/gongbao/content/2002/content_61386.htm.

⁷The introduction dates of the new curriculum were not randomly assigned across provinces. I address the potential selection issue in the robustness check section.

enable me to account for cohort-level and province-level differences that may confound the impact of the curriculum change.

2.3.3 Changes in Textbook Content

A text analysis conducted by Cantoni et al. (2017) reveals that the old and new politics textbooks maintain the same core content, but the new ones significantly shift content on (1) Chinese political institutions, (2) Chinese economic institutions, (3) governance, (4) ethnic identity, and (5) the environment. Given the data availability of the national survey used in this study, my analysis focuses on the first three categories. I discuss each of them below.

Chinese political institutions: the new textbooks emphasize teaching students about socialist democracy, whose core element is political participation under the leadership of the Party. The new politics textbooks encourage students to participate in "democratic elections," advocating that citizens exercise their voting rights when they have the opportunity. In addition, the new textbooks intend to draw a line between orderly (i.e. institutionalized participation) and disorderly civil participation (i.e., unfettered political expression). It notes that citizens' political lives will become chaotic if they ignore the rules, regulations, and procedures put in place by the government.

These changes in the content of Chinese political institutions correspond to the changes in word frequency used in the new textbooks: Compared to the old textbooks, the term *participation* is used 497% more often in the new textbooks. Even greater increases occurred in the frequency of the use of *democracy* (2,057%) and *elections* (4,948%).

Chinese economic institutions: the new textbooks underscore the importance of socialist market economy for economic and social development. In contrast to a free-market economy, it highlights state intervention in the economy. Many newly added sections, such as "Limitations of Market Allocation of Resources" and "Strengthening the State's Macroeconomic Regulations and Controls," deliver the idea that markets are complemented or cor-

rected by government regulation and state institutions. Notably, the new textbooks widely uses everyday citizens' personal economic behavior, such as buying goods and working in labor units, as examples to elaborate why state intervention in citizens' personal lives is imperative for the socialist market economy.⁸ Consistent with these changes in textbook content, the term *government* is mentioned 360% more times in the new textbooks than in the old ones.

Governance: the new textbooks emphasize institutions that legitimize the Chinese government and its officials, especially adherence to the rule of law and administrative supervision. One of the main objectives of teaching students about the rule of law is to promulgate the virtue of "loving the CCP and the nation."⁹ Numerous added sections in the new textbooks note that government officials exert their power and duties according to the law, providing information about the methods by which citizens can supervise the government's power. Reflecting the revisions, the term *legal institution* is mentioned 497% more frequently in the new textbooks than in the old textbooks.

The content changes reflect the objectives that the Chinese government outlined in curriculum reform documents.¹⁰ The content shifts also are consistent with changes that the Chinese government made to the college entrance exam.¹¹ Appendix Section B provides numerous translated excerpts from the new politics textbooks to show how the new textbook content related to the outcome of interest was presented under the new curriculum.

Based on changes in textbook content, I examine three political attitudes: views on state intervention in citizen life, views on democracy, and trust in government officials. If political education works as intended, people studying the new textbooks should be more inclined

⁸Scholars of China's curriculum design have noted that a key feature distinguishing the new curriculum from the old one is that the new politics textbooks use examples from citizens' daily experience to increase the relevance of the textbook materials in the eyes of students (Wang, 2008).

⁹This objective is noted in a memo by the Ministry of Education available at <https://tinyurl.com/yavvosbk>.

¹⁰Cantoni et al. (2017) in their text analysis show that the language used in a curriculum reform document issued by China's State Council is more prevalent in the new textbooks than in the old ones; the specific terms related to the five categories identified show even sharper changes in prevalence across curricula.

¹¹A full item-by-item discussion of each category of interest is presented in Appendix Section B.

than those studying the old textbooks to support state intervention in their personal lives (politically and economically). They should also see people's participation in elections as the defining characteristic of democracy in a more affirmative manner. Finally, they should have higher trust in government officials, who are described as adherents of the rule of law and under administrative supervision.

2.3.4 Data and Variables

The data used in the analysis derive from the Chinese General Social Survey (CGSS), a nationally representative survey run by the Renmin University in China. The CGSS, a part of the International Social Survey Program, is regarded as one of the most professionally managed surveys in China. I use all the available data from four independent waves of the CGSS, one from each of the following years: 2010, 2012, 2013, and 2015.¹²

To implement the identification strategy I discuss below, I analyze only respondents who fulfilled the following criteria. First, their highest level of education completed is at least high school, which removes respondents not attending high school. Second, their high school entry years were close to the years in which the new curriculum was introduced. Third, they either never migrated to other provinces since birth or had migrated to the province in which they currently live before age 15 (i.e., before students begin senior high school). Because the CGSS does not ask where respondents attended high school, I removed those whose migration histories I could not use to infer the places they attended high school.

New Curriculum is the treatment, a binary variable coded as 1 if respondents studied the new politics textbooks and 0 otherwise. Because CGSS does not ask respondents whether they followed the new curriculum or not, I use respondents' birth year to infer their treat-

¹²The analysis begins with the 2010 wave because it is the first round to include a meaningful number of respondents studying the new curriculum. I drop the 2011 wave because it did not ask about the work units of respondents' parents; nor did it measure any outcome variables of interest. The 2014 wave has not been released.

ment conditions under the assumption that students start high school at age 15.¹³ I consider respondents as "treated" if their high school entry year coincides with, or occurs after, the introduction year of the new curriculum. By contrast, if respondents' high school entry year is prior to the introduction year, I consider them as "untreated." Among the 2,092 respondents under analysis, 868 of them were treated (41.49 %), and 1,224 of them (58.51 %) were untreated by the new curriculum.

For the outcome variables, *Intervention* measures respondents' attitudes toward state intervention in citizen life. It aggregates three survey questions measured on a 5-point Likert scale ranging from completely disagree to completely agree. Specifically, the CGSS asks respondents how much they agree with the following statements:

- When an individual criticizes the government in public, the government should not intervene.
- How many children people want to have is a personal matter; the government should not intervene.
- People have the freedom to decide where to work and live; the government should not intervene.

These items indicate different aspects of state intervention in citizen life, including people's political (the first item) and economic lives (the second and third item).¹⁴ Together, this index provides a comprehensive indication of people's general views on state intervention

¹³In the Chinese education system, high school or secondary education is intended for students aged 15 and 18. Granted, not every student starts high school at age 15, but because the CGSS does not ask respondents when they started high school, this assumption is necessary for my analysis. I show in the robustness check section that my results seem not sensitive to this assumption.

¹⁴The birth planning program and the household registration system in China are both important measures that the government uses to serve its economic objectives and macroeconomic control. Thus, the second and third questions are well-suited to measure attitudes toward the role of the government in the economy.

in their personal lives.¹⁵

The next variable is *Democracy*, which measures respondents' views on democracy. It is operationalized using the following question: *A political system can be considered a democracy as long as citizens have the right to elect their representatives, who discuss critical national and local issues on behalf of citizens.* This variable is coded as 1 if respondents agreed with this statement and 0 if not. The next variable is *Trust*, which measures respondents' trust in government officials on a 4-point Likert scale. It is operationalized using the following question: *To what extent do you trust local officials?* I recode these variables in a way that a higher value means greater consistency with the content of the new textbooks. I standardize each outcome variable for ease of comparison.

Because the CGSS may change questions across survey rounds, different rounds may be used to test disparate outcome variables. Specifically, the *Intervention* questions were asked in all four waves. The *Democracy* question was measured only in the 2013 wave. The *Trust* question has fewer observations because only a subset of respondents in the 2012 wave was asked the question.¹⁶

For affiliated students, I use the survey item asking the work units of respondents' parents when the respondents were 14. I define those whose parents (either or both of them) had worked in the Party, the government, or public institutions as affiliated students. If neither of their parents worked in those units, I define them as nonaffiliated students. Notably, people working in Party organs will count as public employees because the Chinese bureaucracy has two parallel lines of authority: the Party and the government. In addition,

¹⁵The Cronbach's alpha estimate is 0.477, which may be acceptable given the limited number of items. For transparency, I report the results using the three items separately in Appendix Section C. Results do not qualitatively change — the estimates are all in the right direction and follow a consistent pattern. I also reestimate the baseline model by using principal component analysis and find that the results are robust.

¹⁶Appendix Section C provides the information about which questions were asked in each wave and the associated number of observation. It also presents additional analyses regarding *Intervention* to address the concern about changes in sampling across waves. For instance, I restrict each model to the smallest number of observations across indicators in that wave. I also include survey year fixed effects in the model to account for time-varying factors that may affect respondents differently across waves.

public institutions in China can be fully or partially state funded or wholly self-funded; that is, not all public employees in public institutions are on the official state payroll. I consider working in the public institutions as working for the government only if the units are at least partially state funded. In total, 11.42% of the respondents are affiliated students. Among these students' parents, 10.67% worked in party-government organs and 89.32% in public institutions. Table 2.1 presents summary statistics of the main variables.

Table 2.1: Summary Statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
Treated (New Curriculum=1)	0.415	0.493	0	1	2092
Affiliated Students (Yes=1)	0.114	0.318	0	1	2092
Intervention (Standardized)	0	1	-2.484	2.72	2075
Intervention (Unstandardized)	8.728	2.306	3	15	2075
Democracy (Standardized)	0	1	-2.18	0.458	536
Democracy (Unstandardized)	0.826	0.379	0	1	536
Trust (Standardized)	0	1	-1.807	1.986	275
Trust (Unstandardized)	2.429	0.791	1	4	275
Gender (Male=1)	0.496	0.5	0	1	2092
Ethnicity (Han=1)	0.940	0.238	0	1	2090
Height (in centimeters)	167.702	7.986	120	192	2090
Residence (Rural=1)	0.218	0.413	0	1	2092
Education	8.93	2.584	5	13	2086
Father in CCP	0.359	0.48	0	1	2071
Mother in CCP	0.297	0.457	0	1	2074
Father Education	4.989	2.31	1	13	2063
Mother Education	4.368	2.228	1	13	2061

Note: This table reports summary statistics of the key variables in this study. Data comes from the Chinese General Social Survey.

The fact that respondents cannot preselect parents employed by the state lessens many sorts of confounders that would affect their political predispositions. In addition, if I follow a common practice used in prior studies that relies on survey items directly asking respondents' political predispositions, a concern is that people's current attitudes almost inevitably affect how they answer these questions. This issue is problematic when respondents' recall of predispositions was influenced by their treatment conditions because having

this variable in the model raises red flags associated with posttreatment biases in what aims to be a causal analysis. By contrast, 14-year-old respondents' treatment conditions cannot affect their parental occupations.

2.3.5 Identification Strategy

Using provincial variation in the curriculum reform and cohort variation in new curriculum eligibility, I estimate a generalized difference-in-differences model as follows:

$$Y_{icp} = \sum_c \gamma_c + \sum_p \delta_p + \beta_1 \text{New Curriculum}_{cp} + \beta_2 \text{Affiliated Students}_{cp} + \beta_3 (\text{New Curriculum}_{cp} \times \text{Affiliated Student}_{cp}) + \epsilon_{icp}, \quad (2.1)$$

where y_{icp} is an individual survey question (i denotes the individual, c the high school entry cohort, and p the province of high school attendance). γ_c and δ_p are full sets of cohort and province fixed effect. The coefficient β_1 captures the treatment effect among nonaffiliated students, conditional on fixed differences across cohorts and provinces of origin. The coefficient β_2 captures the conditional expected values of y among affiliated students who studied the old textbooks; β_3 indicates by how much the effect of the new textbooks changes when respondents are affiliated students. The error terms, ϵ_{icp} , are clustered at the province \times cohort level to account for correlated disturbances across individuals within a province \times cohort cell (the level at which the curriculum varies).

This model can then be used to calculate the marginal effect of the new textbooks on the outcome variables of interest:

$$\frac{\partial(\text{Targeted Attitude})}{\partial(\text{New Curriculum})} = \beta_1 + \beta_3 \text{Affiliated Student} \quad (2.2)$$

Unlike in purely linear models, β_1 has to be interpreted as a conditional coefficient representing the effect among nonaffiliated students (i.e., Affiliated Student = 0). Conversely, the sum of β_1 and β_3 captures the effect among affiliated students (i.e., Affiliated Student = 1). I use listwise deletion to address missing values because the magnitude of missingness is small and will show that my results are robust to imputed data in the robustness check.

To augment the causal inference, I focus on respondents whose high school entry year occurred around the curriculum reform. In the analysis, all respondents come from four cohorts of students who entered high school around the reform year from each side of the curriculum.¹⁷ Table 2.2 describes the birth cohorts of students under analysis and their treatment conditions. Granted, narrowing the bandwidth would avoid more potential biases from selection for treatment, yet because my main hypothesis involves two levels for two factors (i.e., curriculum version and familial ties), narrower bandwidth will result in fewer observations for each combination of the two factors. In the robustness check, I show that the results are insensitive to bandwidth selection.

This identification strategy addresses various methodological concerns. First, province-level differences could confound the influences of the new curriculum because they are likely correlated with people's attitudes. Pan and Xu (2017), for example, find that regional economic composition, including trade openness and urbanization level, correlates with Chinese citizens' ideologies; however, these provincial differences cannot drive my results because I control for province fixed effects and exploit cross-cohort variation *within* provinces.

Second, time-varying provincial heterogeneity could be another concern; for example, differences in economic growth rates across provinces may differentially affect provinces and thus bias the impact of the new curriculum. However, most province-specific, time-varying factors seem unlikely to have very different effects across the neighboring cohorts within a province because the cross-cohort variation exploited falls within a narrow window (i.e., people who entered high school around the reform year). In the robustness check, I employ a tighter identification in which the interaction terms between province and cohort

¹⁷To illustrate, I use Shandong as an example. Because the new curriculum was introduced in 2004, the first entry cohort of students receiving the new textbooks comprised those born in 1989, and the last entry cohort of students receiving the old textbooks comprised those born in 1988. For Shandong, the analysis focuses only on the cohorts of students born between 1985 and 1992, defining those born between 1985 and 1988 as the control group (the last four prereform cohorts) and those born between 1989 and 1992 as the treatment group (the first four postreform cohorts).

Table 2.2: Reform Years Across Provinces and Cohorts Under Analysis

Years	Provinces Under Reform	Cohorts Analyzed
2004	Shandong, Ningxia, Hainan, Guangdong	1985-1988 (C) 1989-1992 (T)
2005	Jiangsu	1986-1989 (C) 1990-1993 (T)
2006	Tianjin, Zhejiang, Fujian, Anhui, Liaoning	1987-1990 (C) 1991-1994 (T)
2007	Hunan, Jilin, Shaanxi, Heilongjiang, Beijing	1988-1991 (C) 1992-1995 (T)
2008	Shanxi, Jiangxi, Henan, Xinjiang	1989-1992 (C) 1993-1996 (T)
2009	Hebei, Hubei, Yunnan, Inner Mongol	1990-1993 (C) 1994-1997 (T)
2010	Sichuan, Gansu, Guangxi, Qinghai, Tibet, Chongqing, Guizhou	1991-1994 (C) 1995-1998 (T)

Note: Year refers to dates of introduction of the new textbooks. Provinces refer to the locations in which the reform was implemented. Cohorts Analyzed describes the treatment conditions of the birth cohorts analyzed, with C denoting control group and T denoting treatment group.

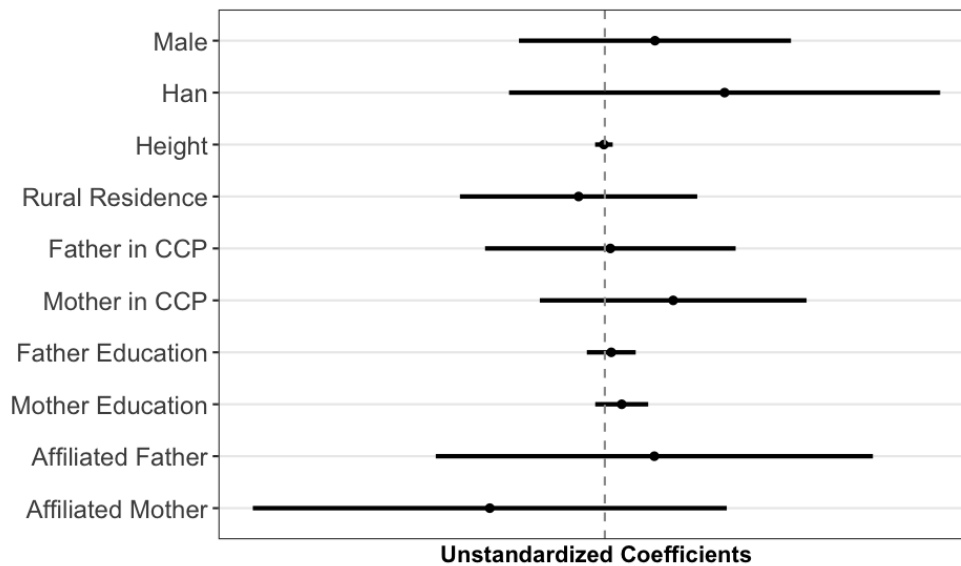
fixed effects are included. Such model specification helps me to address the unobserved province and cohort covarying characteristics.

Third, the natural evolution of attitudes across cohorts of students may explain attitudinal differences between treated and untreated students even in the absence of the new textbooks. This concern, however, is alleviated by including cohort fixed effects in the model that can zero out cross-cohort changes in attitudes. Fourth, one could argue that the curriculum reform might accompany other policies introduced to a reformed province; the policies could bias the results if they differentially affected the treated and untreated respondents. I consulted newspapers and no such policy exists.

2.4 Results

I first show that respondents under analysis who followed the old and new textbooks are statistically indistinguishable. Because respondents studying the new textbooks are younger and come from provinces where the new curriculum was introduced earlier, I include province and cohort fixed effects in the balance check to account for common characteristics in the province of origin and average characteristics of a cohort. Figure 2.1 shows that the treated and untreated respondents are similar in a battery of personal characteristics. Appendix Section D reports the full estimates.

Figure 2.1: Balance Test

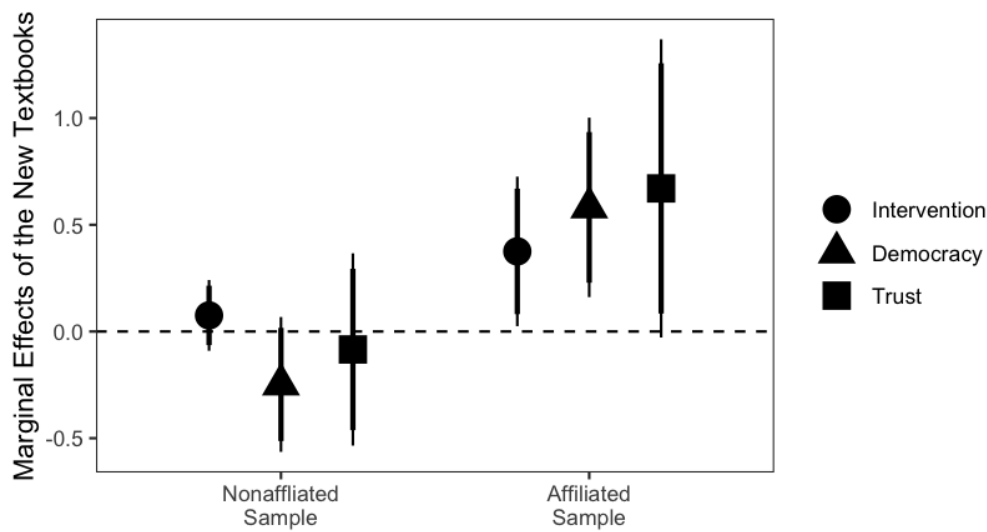


Note: This coefficient plot shows the OLS estimates of personal characteristics on exposure to the new curriculum. The dots represent the regression coefficients and the bars 95% confidence intervals. All regressions include a full set of province and cohort fixed effects. N=2026.

The main result supports my argument that the effectiveness of state indoctrination is conditional. Figure 2.2 reports the estimated marginal treatment effects for affiliated and nonaffiliated students with corresponding confidence intervals. It shows that the new textbooks successfully affected only affiliated students. Compared to the untreated affiliated students, affiliated students exposed to the new textbooks exhibit more positive attitudes toward government intervention in citizen life ($p \leq 0.05$), view participation in elections

as a defining characteristic of democracy ($p \leq 0.05$), and feel greater trust in local officials ($p \leq 0.1$). By contrast, the new textbooks did not affect nonaffiliated students in the same manner. Among these students, those studying the new textbooks seem indistinguishable across the three political attitudes that the government aimed to change from those studying the old textbooks. Some coefficients even have a wrong sign: the estimates for *Democracy* and *Trust* are negative, although they are not statistically significant.

Figure 2.2: Marginal Effects of the New Textbooks



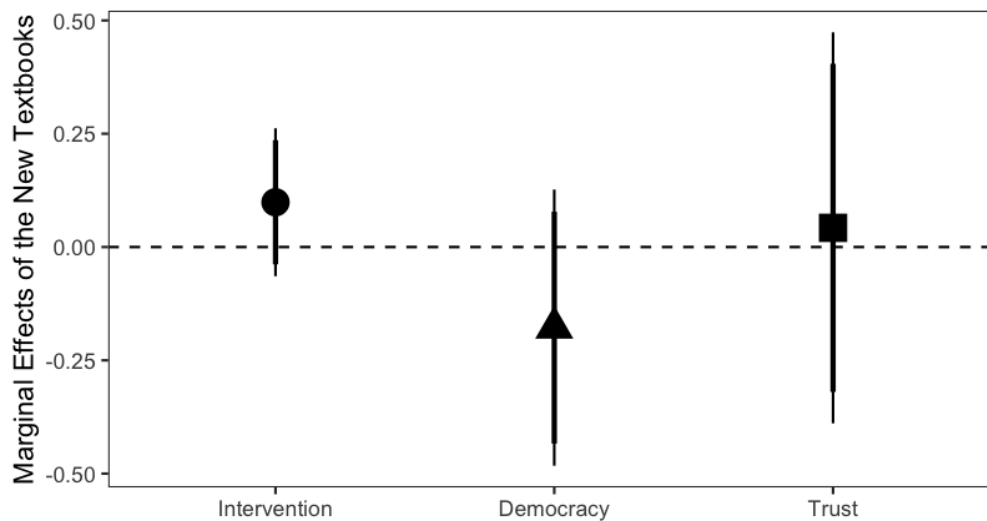
Note: This plot shows the effects of the new textbooks on the targeted attitudes by affiliation status. The bullet symbols represent the standardized coefficients and the bars 95% (90%) confidence intervals. All regression coefficients account for province and cohort fixed effects. Standard errors are clustered at the province \times cohort level. N=2075 (Intervention); N=536 (Democracy); N=275 (Trust)

Moreover, the estimates of the interaction variable are statistically significant at the .1 level, meaning that the treatment effects on affiliated students seem larger than those on nonaffiliated students (see Appendix Section D). Note that my argument about the conditional indoctrination does not necessarily mean that the new textbooks had larger effects on affiliated students than on nonaffiliated students, yet such evidence bolsters my argument that the effectiveness of state indoctrination depends on receivers' familial connections to the regime.

One should also expect that the overall effects of the new textbooks are indiscernible because the vast majority of the respondents analyzed in this paper are nonaffiliated students. This expectation is consistent with Huang (2015a), who finds that political education in Chinese colleges does not indoctrinate students. To test this claim, I remove the conditioning variable and the interaction term from the baseline model, which allows me to interpret the regression coefficients associated with *New Curriculum* as the average effects of the new textbooks on students' attitudes.

The OLS estimates in Figure 2.3 support my expectation, showing that overall the new textbooks had no demonstrable effect on people's political attitudes. In addition, the coefficient signs are unstable: although the estimate for *Intervention* and *Trust* is positive, it is negative for *Democracy*. In short, I find that the new textbooks did not persuade the majority of people in a manipulative fashion, casting doubt on the actual degree to which the Chinese regime can change minds by changing school content. Appendix Section D presents the full estimates.

Figure 2.3: Average Treatment Effects



Note: This plot shows the estimated effects of the new textbooks using the pooled sample. The bullet symbols represent the standardized coefficients and the bars 95% (90%) confidence intervals. All coefficients are OLS estimates and account for province and cohort fixed effects. Standard errors are clustered at the province \times cohort level. N=2075 (Intervention); N=536 (Democracy); N=275 (Trust)

My finding that indoctrination works only for children of regime stakeholders is not trivial. One might contend that these students may have been fairly aligned with the government in attitudes that the Chinese authorities attempt to change, so making them more aligned with the government seems unimportant; yet the data shows that the untreated affiliated students have significantly *lower* scores on the three targeted attitudes than the untreated nonaffiliated students (not reported). The benchmark differences indicate that my results are consequential.

2.4.1 Falsification Tests

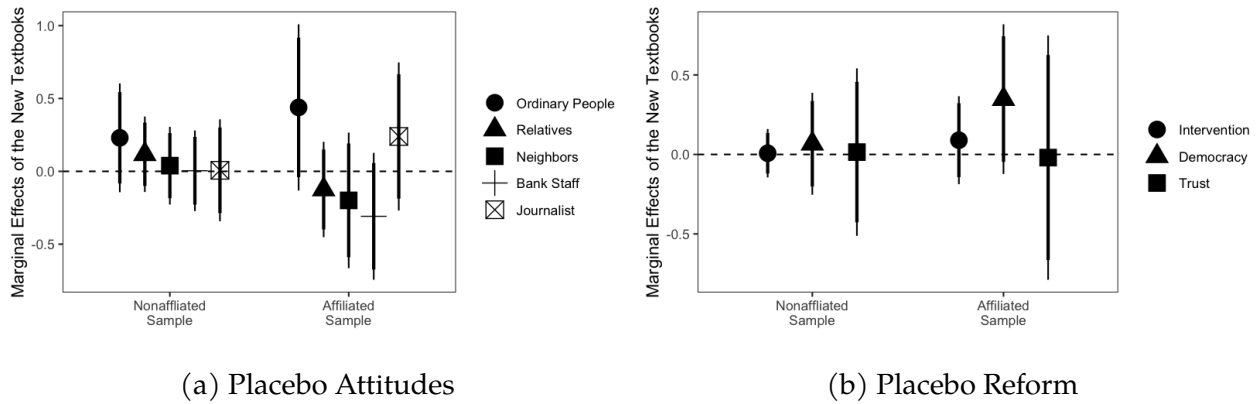
To corroborate the treatment effect identification, I conduct two falsification tests. First, I reanalyze the data using attitudes not targeted in the new textbooks. Because the placebo attitudes were not what the new textbooks aimed to change, one should observe no effect. I select five attitudes related to trust (ordinary people, relatives, neighbors, bank staff, and journalists). Panel (a) in Figure 2.4 shows that the new textbooks had no impact on the placebo attitudes, even among the affiliated student sample where I find indoctrination works. Second, I reanalyze the data by moving the introduction dates of the new curriculum three years before its actual dates. That is, the last three cohorts of students studying the old textbooks in real life are considered here as the first three cohorts studying the new textbooks. Under the placebo reform years, none of the cohorts of students analyzed was exposed to the new textbooks, so no effect should occur. Panel (b) in Figure 2.4 shows that the effects identified previously disappear in the falsification test.

2.4.2 Robustness Check

I conduct seven sets of additional analysis to show that my results are robust.¹⁸ First, I examine whether the results are sensitive to the assumption that students start high school at age 15. Second, I address the concern that parental occupation is endogenous. Third,

¹⁸In Appendix Section E, I use tables to report the full results based on the baseline model and coefficient plots to show the marginal treatment effects by affiliation status.

Figure 2.4: Falsification Tests



Note: Panel (a) shows the effects of the new textbooks on nontargeted attitudes. Panel (b) presents the effects of the new textbooks using placebo reform. The bullet symbols represent the standardized coefficients and the bars 95% (90%) confidence intervals. All regression coefficients are OLS estimates and account for province and cohort fixed effects. Standard errors are clustered at the province \times cohort level. Appendix Section D reports the regression coefficients.

I reanalyze the data by controlling for individual-level covariates. Fourth, I address the concern that the introduction dates of the new curriculum were not randomized across provinces. Fifth, I reanalyze the data by taking province-specific, cross-cohort trends into account. Sixth, I use a more demanding model to address the unobservable province and cohort covarying characteristics that could bias the results. Seventh, I reanalyze the data using multiple imputed data for missing values. Appendix Section E discusses these analyses in more detail and reports the results.

2.5 Alternative Explanations

I discuss four alternative explanations of the effects. First, affiliated students may tend to falsify their preferences (Jiang and Yang, 2016; Truex and Tavana, 2019): they may be more likely than nonaffiliated students to express politically correct views as suggested by the new textbooks. If so, the effects are detected even when affiliated students are not truly indoctrinated by the textbooks' content. Second, affiliated students may pay better attention to political education or are more academically competent than nonaffiliated students.

The effects may thus reflect the fact that they memorize lines from the content of the new textbooks better rather than believe it.

Third, a complementarity may exist between family influence and school education: the new textbooks alone may not be able to affect affiliated students unless the textbooks' content is complemented by family influence. For example, state-related parents may discuss with their children the exact same lessons the new textbooks purposely emphasized, "teaching" the textbook content outside the classroom. The content of the new textbooks is reinforced by such family influence to indoctrinate affiliated students.¹⁹ Fourth, the introduction of the new curriculum was likely accompanied by better educational resources allocated to students, such as improved textbook quality, teaching practice, and school spending. Consistent with the results, the textbook effects can be conditional on predispositions because affiliated students may respond more positively than nonaffiliated ones to government actions that benefit them. If so, the effects may reflect changes in educational quality rather than changes in educational content.

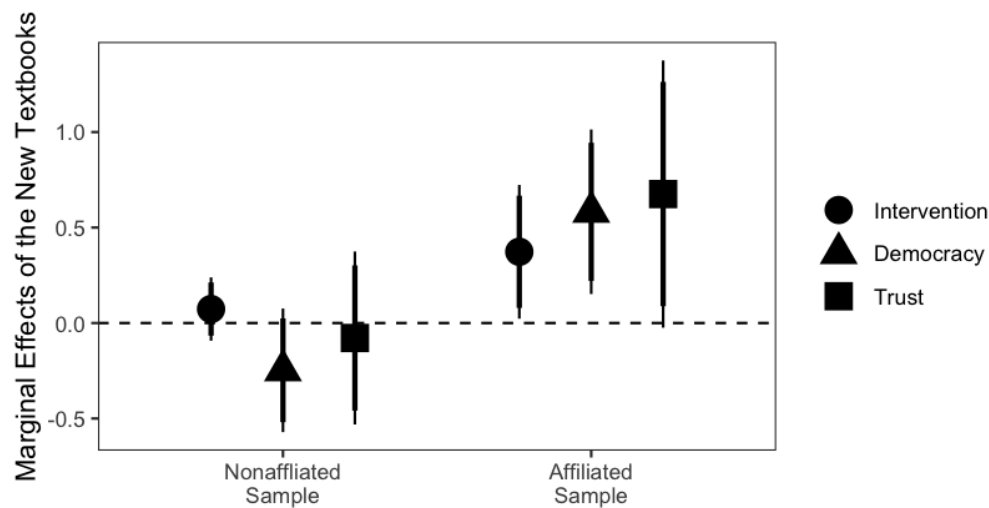
The first three alternative explanations put a slightly different spin on the interpretations of the effects but may not change the treatment effect identification *per se*. By contrast, if the fourth alternative explanation is true, my estimates could be biased because it suggests that a confounder (i.e., educational quality), rather than the claimed treatment (i.e., educational content), caused my results. Two analyses show that this alternative explanation is unlikely to explain my finding.

I first reanalyze the data by controlling for provincial spending on secondary education at the province-cohort level (calculated as a province's average level of spending during the three years of senior high school for each cohort). Figure 2.5 shows that the estimates

¹⁹ Although I cannot rule out this explanation directly, I have suggestive evidence to refute another related claim: affiliated students may have been affected by the new curriculum in the family before they attend high school and thus the results may be better interpreted as the effects of "new curriculum parents," not the new curriculum *per se*; yet if the effects are based on prehigh school indoctrination, affiliated and nonaffiliated respondents who did not attend high school around the curriculum reform years should express different targeted attitudes. Results reported in Table A.15 do not support this conjecture.

controlling for school spending are nearly identical to the baseline results. Even if school spending may have been greater for the cohorts in provinces exposed to the new curriculum, better educational quality accompanying the curriculum reform does not drive the result.

Figure 2.5: Marginal Effects of the New Textbooks Controlling for Spending on Secondary Education at the province \times cohort level



Note: The bullet symbols represent the standardized coefficients and the bars 95% (90%) confidence intervals. All regression coefficients include a full set of province and cohort fixed effects. Standard errors are clustered at the province \times cohort level.

To complement the quantitative evidence, I also conducted semistructured interviews with high school teachers and graduates from Beijing, Fujian, and Sichuan in China. My interviews reveal that students always get brand new textbooks each year even in the old design. This is true in all urban and rural areas; the only exception is perhaps extremely poor regions. In other words, textbooks in the old design do not mean that they are dilapidated books; nor did the teachers think teaching practices were substantially changed as a result of the reform because teachers' and students' incentives were still directed toward the memorization of textbook content to succeed in the college entrance exam. This view concurs with observations from many Chinese scholars of curriculum design and public education (Guo, 2010; Mao, 2018). The qualitative evidence could alleviate the concern that

improved textbook quality and teaching practice may confound the effect of the curriculum change.

2.6 Conclusion

This paper finds consistent evidence that the new politics textbooks worked only among respondents whose parents had been employed by the government. By contrast, the same state indoctrination effort had no discernible impact on the vast majority of the respondents.²⁰ In light of the findings of Cantoni et al. (2017) that the new politics textbooks were successful in shifting people's political attitudes in an intended direction, the results of this paper seem to disagree.

My results, however, are fairly compatible with theirs once taking their survey respondents' family backgrounds into consideration. Their student sample comes from Peking University (PKU), in which the most common occupation type among the students' parents since 1997 is cadres (*ganbu*) in party-government organs and public institutions (Liang and Lee, 2012).²¹ Because many parents of PKU students have close ties to state patronage, their sample seems likely to show the effect of the new textbooks. My finding not only corroborates their study but also casts doubt on the degree to which the Chinese government can change minds by changing school textbooks on a large scale.

Besides the different sample adopted, two main differences between this study and that of Cantoni et al. (2017) may explain why I find no effect on the general population. The first pertains to the difference in reform years covered: Cantoni et al. (2017) look only at students from provinces where the reform occurred in 2007, 2008, and 2009 because they

²⁰The null effect may be a surprise because the Chinese government owns a massive institutional structure and resources for propaganda provision (Brady, 2009; Shambaugh, 2007). Besides, propaganda should readily work because most Chinese citizens have comparatively limited access to competing messages (Stockmann and Gallagher, 2011). Finally, when the respondents were exposed to political education, they were in their formative years.

²¹According to their data, 39.76% of PKU students' parents from 1995-1999 were cadres in party-government organs and public institutions. Only 1.7% of the Chinese population were cadres in these work units in 2000.

surveyed only students in college. This paper, by contrast, covers the entire time period of the reform. If provinces that introduced the new curriculum later did better in the implementation of the reform because they learned from the provinces that implemented earlier, Cantoni et al. (2017) may have much leverage to find the effect. I reanalyze the data with only these latecomer provinces but still find no effect on the general population. The second reason relates to the duration of effects: because Cantoni et al. (2017) surveyed only college students, the case could be made that an effect for respondents in college indeed exists and that the effect decreases over time for those out of college for longer periods of time. Given the data at hand, I cannot evaluate this hypothesis. The only statement I can make is that the effect on those who have familial ties to the regime seems lasting over time.²²

One limitation of this paper is that the CGSS does not include items measuring respondents' family dynamics that would affect transmission rates. According to the extant literature, the transmission of political predispositions from parents to children is higher in families where political discussion among family members is frequent (family politicization). Parent-to-child transmission rates are also higher when the family forms a collective body, not a mere assemblage of fortuitously related individuals (unity). This paper is focused primarily on the direction of political attachments (i.e., attached to the CCP), but the intensity of political attachments resulting from family politicization and unity could also be relevant. I leave this empirical inquiry to future researchers.

Despite limitations, this paper contributes to existing scholarship in three chief ways. First, my finding is not only consistent with new studies showing that hard propaganda has a limited effect on attitudes (Bush et al., 2016; Huang, 2015a, 2018; Selb and Munzert, 2018) but sheds light on what segment of the population on which propaganda works

²²This claim is based on the fact that many respondents in this study have graduated from college. I further explore this issue by focusing on the early cohorts studying the new textbooks. Results based on the sample with cohorts just one year above and below the reform year show that the effects are still discernible among the affiliated students who constitute the earliest cohort in their province to study the new curriculum. This is stronger evidence of the duration of treatment effect. Appendix Section E report the results.

best. Second, my finding highlights the importance of people's familial ties to the regime in moderating their responses to propaganda, suggesting that the political contexts in which people were socialized may affect their receptivity to propaganda. Future research can examine the micro-level mechanisms underlying the results.

Finally, my finding suggests an alternative account for reasons that authoritarian governments still engage in propaganda when most citizens seem not to believe it. The prevailing view is that propaganda signals government strength in maintaining social control and political order (Huang, 2015a; Wedeen, 1999). I add that political elites invest resources in state indoctrination because it could reinforce existing believers. This claim also speaks to a new body of research on the presence of propaganda as a strategy of control (Chen and Xu, 2015; Guriev and Treisman, 2015). My finding implies that state indoctrination could make a regime durable not because it expands the popular base of the regime but because it consolidates those already aligned with the government.

Chapter 3

How the Pro-Beijing Media Influences Voters: A Field Experiment

3.1 Introduction

Overseas media operations are ubiquitous. In both democracies and autocracies, many governments have devoted considerable resources and efforts to influencing the public outside their borders. Prior studies focus mainly on the political impact of foreign media in autocracies (Crabtree, Darmofal and Kern, 2015; Crabtree, Kern and Pfaff, 2018; Kern, 2011; Kern and Hainmueller, 2009; Krugler, 2000). Some recent work has investigated foreign media's cross-border effects on democratic elections (DellaVigna et al., 2014; Peisakhin and Rozenas, 2018). Yet, no media influence receives more attention today than the overseas media operations deployed by powerful autocratic countries like China and Russia (Chapman and Gerber, 2019; DiResta et al., 2020; Fisher, 2020; McCabe, 2020).

China views political persuasion and information management as a top government priority (Brady, 2009). In recent years the country has engaged in global information campaigns to promote the country's positive image and political agenda. China's overseas media operations take various forms, including coopting foreign media outlets (Dai and Luqiu, 2020; Hamilton, 2018; Hsu, 2014; Sciutto, 1996), expanding its state-owned media networks (Bailard, 2016; Gorfinkel et al., 2014; Wasserman and Madrid-Morales, 2018), and using social media (Min and Luqiu, 2020) to disseminate Beijing's preferred messages abroad.

As for China's global information campaigns, Taiwan is an important target. From its inception in 1949, the Chinese Communist Party (CCP) has claimed the island is part of China's sovereign territory. To deter Taiwan from pursuing independence, China has invested heavily in Taiwan's media outlets to influence news coverage on the island (Hsu,

2014; Huang, 2017). Reports show that China has given a Taiwan media outlet, *The China Times*, editorial instructions on how China–Taiwan issues should be covered (Aspinwall, 2019; Hille, 2019; Hsu, 2014). The CCP also provides funds to media outlets that adopt a pro-Beijing line in their reports (Huang, 2019b; Lee and Cheng, 2019). China’s involvement in Taiwan’s media market has led to mass protests against "red media," that is, Taiwan-based media that actively fall in line with Beijing’s interest.

Concerns about China’s interference in elections through friendly media have become salient in recent years, but the effectiveness of pro-Beijing media in affecting voters remains unknown because there is little well-identified evidence. The effect of exposure to pro-Beijing media is not a priori obvious. Some writers have claimed that increasing access to China-friendly information will sway receivers in China’s desirable direction (Huang, 2019c). Previous studies also suggest that most media consumers do not discount media bias strongly enough and are thus subject to persuasion upon exposure (Cain, Loewenstein and Moore, 2005). By contrast the impact of pro-Beijing media could be trivial because people may be certain about the bias of the media and thus exposure will have no effect on beliefs and voting (Durante and Knight, 2012). The limited impact of the political media may be particularly true in democracies in which media consumers exercise greater choice over both media content and sources (Arceneaux and Johnson, 2013). Still others argue that increasing the supply of such media messages among voters will not lead to a greater convergence of beliefs: the study of confirmatory bias shows that giving additional information to people with different prior opinions can lead to divergence rather than convergence of beliefs (Levendusky, 2013).

Identifying the effect of pro-Beijing media on individuals’ voting behavior and political attitudes is challenging. Most studies of the effects of the media are limited by their dependence on survey methods and observational data. In this tradition the standard test for media effects is the difference in outcomes for individuals who report high levels of media exposure and those who report low levels. This design has two major problems. First, peo-

ple’s political media use is often endogenous to their political preferences (Bartels, 1993; Stroud, 2008): those who choose to tune in to pro-Beijing media may differ systematically in ways that matter to their vote choices and opinions about China from those who do not. This selection issue vitiates the ability to disentangle cause from effect. Despite recent advances in the design of observational studies (DellaVigna et al., 2014; Peisakhin and Rozenas, 2018), they cannot identify the causal impact of media communication without invoking assumptions about the unobservables. Second, relying on self-reported media use and news consumption can lead to biased conclusions due to faulty recall, social desirability concerns, and other sources of misreporting (Prior, 2013; Guess, 2015).

To address the methodological hurdles, I use a field experiment that randomly assigns study participants to receive real-time political news coverage from *The China Times*.¹ I present the news coverage on a website and incentivize participants to browse the website in the weeks leading up to the 2020 Taiwanese presidential election. To probe whether and how much participants consume news coverage from the website, I use web traffic data to track their browsing behavior on the website. Compared to self-reports, the tracking data provides much more accurate information on individuals’ exposure patterns.

A panel survey was conducted to measure people’s voting decisions and attitudes toward China before and after the experiment. I fielded a baseline survey four weeks prior to the election, giving random participants access to the news website. After the election, I recontacted all participants for an endline survey. By combining the survey data and tracking data, I examine the individual-level changes in outcome scores as a result of exposure to the pro-Beijing news. The survey also contains a rich set of participants’ background characteristics, allowing me to evaluate effect heterogeneity.

Results show that pro-Beijing news has a direct impact on people’s candidate choices. Those who are randomly assigned to receive the news website become more likely to vote

¹I preregistered this experiment with EGAP (ID 20191221AA) and obtained approval from the IRB of the University of Texas at Austin under protocol 2019-09-017.

for China's preferred presidential candidate. In substantive terms, exposure to the news coverage moves from 15.9 to 26.8 percent (depending on the definition of compliance) of voters who were not already persuaded to choose China's preferred candidate on election day. Importantly, the effects are realized mainly by persuading undecided voters and bringing partisans home rather than by turning voters away from their initial vote intention.

The same political stimulus has differential effects on voters with opposing political priors. Although the pro-Beijing media in this study on average had an intended effect on voters' favorability toward China and its preferred candidate, these results are predominantly driven by people who are predisposed to accept China-friendly messages and those who are nonpartisans. By contrast the same pro-Beijing news had a negligible, even backfire, effect among those who had been more China-skeptical *ex ante* and those who think the pro-Beijing media outlet in this study is connected with the Chinese government.

More evidence suggests that the results work through persuasion: the treatment effects are significantly larger among voters who are less attentive to the 2020 election in the baseline survey and those who think the pro-Beijing media outlet is a credible news source. These results are consistent with models in which people with lower stored information are more susceptible to media messages (Zaller, 1992). They also align with studies showing that persuasive messages work best when their perceived credibility is high (Lupia and McCubbins, 1998). I further evaluate why the pro-Beijing media influences voters, finding that pro-Beijing news triggers people's cognitive and emotional reactions in the direction consistent with the way the treatment changes their behavior and attitudes. A placebo test confirms the findings.

This paper offers two main contributions. First, it is the earliest investigation that analyzes the effectiveness of China's overseas influence operations in swaying voters. My field experiment provides evidence that a main pro-Beijing media outlet successfully affects the public outside mainland China in Beijing's favor. Second, this paper presents and implements a new approach to documenting political media effects in real-world settings through

the combination of field experiment and observed behavior of media consumption.

My study differs from those exploring media effects in laboratories (Benedictis-Kessner et al., 2019; Levendusky, 2013); it also differs from studies examining only attitudes (Arceneaux and Johnson, 2013; Benedictis-Kessner et al., 2019; Bleck and Michelitch, 2017; Druckman, Peterson and Slothuus, 2013). Even if some scholars examine voting behavior, their analyses are based on either aggregate data (Adena et al., 2015; DellaVigna and Kaplan, 2007; DellaVigna et al., 2014; Martin and Yurukoglu, 2017) or self-reported media exposure (Peisakhin and Rozenas, 2018; Gerber, Karlan and Bergan, 2009). In contrast to these studies, in the current study I use individual-level data with information on individuals' media consumption behavior in real-world settings.

The findings of this paper have important implications for current events regarding not only the general issue of foreign interference in democratic elections but also the rise of China and its influence on neighboring countries—perhaps even its global reach.

3.2 Theoretical Expectations

Citizens learn about politics and government from mass media (Graber and Dunaway, 2017; Paletz, 2002). The role of the media is most evident at election times, when the media are the primary conduits of information between candidates and voters. Because few people attend rallies or have direct contact with the candidates or their representatives, most voters have incomplete information about candidate quality and policy positions. The media thus provide the bulk of information voters can use in elections. Voters form or update their evaluations of candidates through media-based information (Dalton, Beck and Huckfeldt, 1998; Gelman and King, 1993; Kahn and Kenney, 2002).

Consistent with these arguments, recent empirical work provides evidence of media effects on voters' evaluation and choice of candidates (Adena et al., 2015; DellaVigna and Kaplan, 2007; Druckman and Parkin, 2005; Huber and Arceneaux, 2007; Ladd and Lenz, 2009; Martin and Yurukoglu, 2017; Murphy and Devine, 2018). These studies align with

some early work (Barker, 1999; Bartels, 1993; Noelle-Neumann, 1974; Zaller, 1996) in the sense that they posit that the media have a direct influence on the public by reinforcing people's existing opinions or persuading them to support particular candidates or political parties.

I argue that pro-Beijing media affect behavior because they change receivers' opinions. Because pro-Beijing media messages are slanted in favor of China and its preferred candidate, individuals receiving such information over time would become more positive toward China and its preferred candidate, which in turn should increase their likelihood of choosing the favored candidate. Thus, the first hypothesis is as follows:

Hypothesis 1 (direct media influence): *On average, pro-Beijing media messages will nudge people to vote for China's preferred presidential candidate and adopt more favorable attitudes toward China.*

This line of reasoning contains three testable hypotheses. First, the effects of pro-Beijing media should be greater among voters who are less attentive to politics *ex ante*. Persuasion tends to be more effective when receivers have less information. The weaker the receivers' priors, the more new information affects their beliefs. By contrast behavior will be less elastic when receivers are close to certain about the state *ex ante* (DellaVigna and Gentzkow, 2010). Research on campaign persuasion has shown that voters will discount new information relative to their existing stories of politically relevant information. Studies have also found that highly informed citizens are more resistant to changing their political views after exposure to state propaganda (Geddes and Zaller, 1989; Stockmann and Gallagher, 2011). In short, the lower this inertia resistance, the greater the susceptibility of voters to alter their beliefs in the face of persuasion (Zaller, 1992). This leads me to the second hypothesis:

Hypothesis 2 (political attentiveness): *The effects of pro-Beijing media will be greater among voters who are less attentive to politics before exposure to pro-Beijing media messages.*

Additionally, the pro-Beijing media should be more effective in shifting behaviors among voters who think the information is credible because people's inferences from a given message will depend on what they know about the credibility of the message source (Chaiken and Maheswaran, 1994; Lupia and McCubbins, 1998; Pornpitakpan, 2004). Previous studies have shown that people confronted with information from a source known to be biased would account for this bias in their learning (Chiang and Knight, 2011). The third hypothesis is thus as follows:

Hypothesis 3 (source credibility): *The pro-Beijing media effects will be more pronounced among voters who think the pro-Beijing media provides credible information.*

Furthermore, every opinion is a marriage of information and predispositions (Zaller, 1992), and thus pro-Beijing media could differentially influence people with opposing pre-existing political preferences. When processing new information on hot cognition issues (e.g., politics), people are often motivated to uphold their prior beliefs (Kunda, 1990; Taber and Lodge, 2006), making them easily assimilate information congruent with their priors but discount, even counterargue, information that challenges their priors (Ditto and F. Lopez, 1992; Lord, Ross and Lepper, 1979; Taber and Lodge, 2006). As a result exposure to the same political information may not converge but instead alter people's beliefs in differential directions because they learn from the information differently in accordance with their political predispositions. Like other political media programs (e.g., Levendusky, 2013), the pro-Beijing media could trigger and intensify such biased reasoning because of the media outlet's slanted presentation of the news and its one-sided messages.

In particular, pro-Beijing media may have backfire effects among voters who are dismissive of China *ex ante*. Taber and Lodge (2006) and Redlawsk (2002) interpret backfire effects as a possible result of the process by which people counterargue preference-incongruent information and bolster their preexisting views. If people counterargue unwelcome information vigorously enough, they may report opinions that are more extreme than they oth-

erwise would have had. Some recent studies show evidence of backfire effects (e.g., Adena et al., 2015; Bail et al., 2018; Levendusky, 2013; Nyhan and Reifler, 2010); yet others yield opposite results (Guess and Coppock, 2018). Together, this leads to my fourth hypothesis:

Hypothesis 4 (prior political preferences): *The pro-Beijing media will have positive effects among voters who are predisposed to be China-friendly but will have negative effects among those who are China-skeptical ex ante.*

3.3 Political Landscape in Taiwan

Taiwan was an authoritarian regime from the time the Kuomintang (KMT) arrived on the island from mainland China in 1949 until its loss of the presidency in 2000. Through the enforcement of martial law and political machine, the KMT kept a powerful hold on the state and throughout the Cold War (Rigger, 2000). National elections were suspended in the name of national emergency arising from the confrontation with the CCP. Any perceived opposition to the KMT was considered illegal and repressed. The opponents operated under the informal rubric of "dangwai," or outside the party. In 1986, dangwai politicians founded the Democratic Progressive Party (DPP). Although this move technically violated martial law, the event went unpunished, and only ten months later, the KMT terminated martial law and allowed opposition parties to emerge (Fan and Feigert, 1988; Chao and Myers, 1998). Taiwan's national legislative bodies were under complete reelection in 1992 and its first direct presidential election was held in 1996. The DPP's electoral performance between 1986 and 2000, however, was stagnant because the KMT still preserved a large popular base and resource advantage (Greene, 2007; Rigger, 2000). The DPP won the presidency thanks to a divided KMT in the 2000 election, ending more than half a century of KMT rule on Taiwan.

The dominant cleavage in Taiwan's presidential elections is organized around policy on Beijing. Accordingly, the political scene is divided into two camps (Schubert, 2004). Led by the KMT, the pan-Blue camp is friendlier to Beijing and believe that expanding

economic ties with China is important for Taiwan's continued economic dynamism. The pan-Green camp led by the DPP, by contrast, argues that increasing these ties threatens national sovereignty and security. The pan-Green camp thus adopts a more skeptical stance toward Beijing and advocates a Taiwanese national identity distinctive from the mainland. Relations with China dominate Taiwan's domestic political discourse and form the main political and social cleavage separating the two major parties (Clark and Tan, 2012).

Despite the differences, a majority of Taiwan's voters evades directly factoring in the choice of unification or independence, at least not immediately. Most voters consider an open-ended future of the relationship with the mainland as the best option of Taiwan, which can own the benefits of *de facto* independence (economic freedom and democratic self-government) without the risk of *de jure* independence (Chu, 2004; Rigger, 2001). Consequently, both political camps state a desire to maintain the status quo. According to Taiwan's Election and Democratization Study, a continual large-scale survey research project, the share of independent voters in the population has steadily increased since 2011; in 2019, around 40% of voters are self-identified as nonpartisans, who tend to exhibit a moderate position on various issues germane to China-Taiwan relations (Wang, 2019).

The 2020 presidential election took place on January 11 to elect the president and all members of the legislature. The election had a turnout of 74.9%, up from 66% four years earlier and the highest among nationwide elections since 2008. Three major presidential candidates ran in the election: Tsai Ing-Wen of the DPP, who was elected in 2016 and sought a second term; Han Kuo-yu of the KMT, who was elected Mayor of Kaohsiung in 2018; and James Soong, who is the chairman of a third-party in the Blue camp. Tsai won the election with 57.13% of vote share. Han was the runner up with 38.6% of vote share; Soong came third with 4.26% of the vote.²

²Given that the election was not a close one, this study was unlikely to affect the election outcome and thus I had no ethical concern. Besides, I show in a following section that the study did not decrease turnout, a practice deemed essential for any well-functioning democracy. The finding eases the concern that my experiment may demobilize people to vote.

3.4 Experimental Design

The experimental design is summarized in Figure 3.1. Prior to treatment assignment, I identify in a baseline survey conducted four weeks before the election those study participants who are already existing China Times (CT) consumers.³ I exclude these existing consumers in the subsequent treatment assignment but follow them throughout the study because they serve as benchmarks to interpret the treatment effects.⁴ The existing consumers receive the same survey questionnaire as those who are not existing consumers. Next, I randomly assign the remaining participants to one of the three groups: (1) treatment group, members of which receive access to a website containing real political news from *The China Times*; (2) placebo group, in which people receive access to another website containing real entertainment news from *The China Times*; and (3) control group, in which people are subject to the media environment as in the status quo (i.e., receive no website; no exogenous source of news). I conduct the treatment assignment upon the completion of the baseline survey.

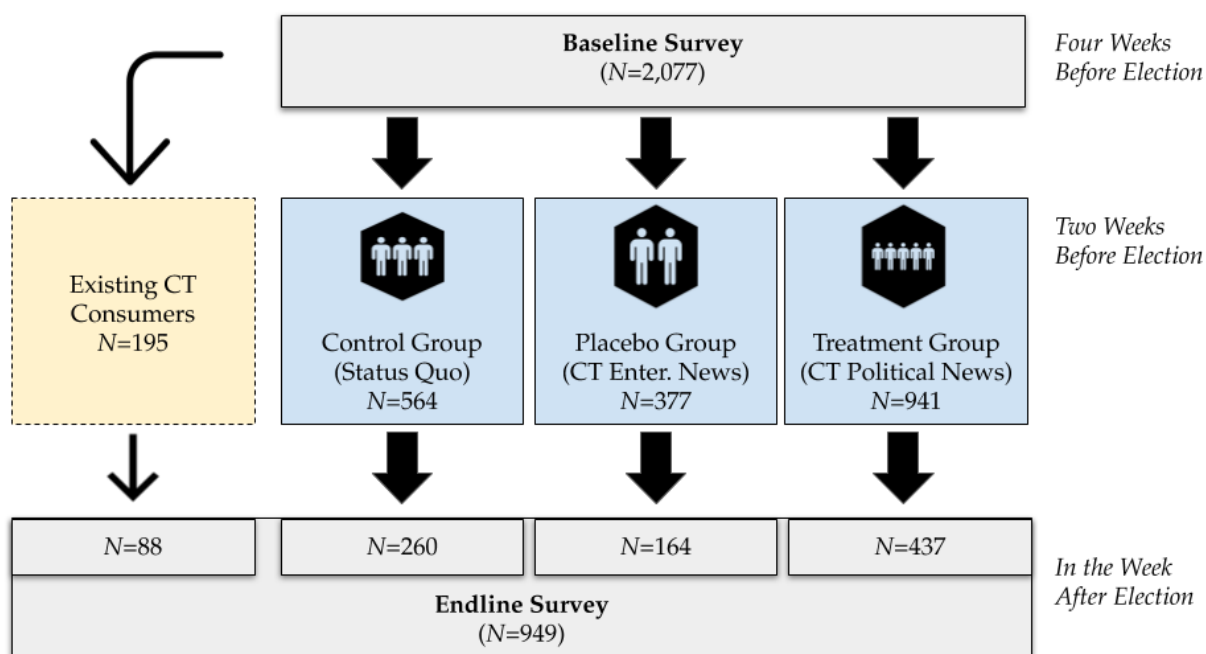
Participants have website access during the two weeks leading up to the election (i.e., from December 28, 2019, to January 11, 2020). On December 28, 2019, participants receive my first email about the website.⁵ I incentivize participants to visit a website containing important daily news (see B.1 for a screenshot of the invitation email in Online Appendix). Participants are told that they will receive NT\$150.00 (equivalent to \$5.00) if they spend an average of three minutes per day browsing the website in the days leading up to January 11, 2020. I attach a user-specific hyperlink to the email for participants to access the website.

³I identify existing China Times consumers by using a survey question asking respondents whether they read any of Taiwan's four largest newspapers in Taiwan on a nearly daily basis. If participants answer no, they are in the experimental sample. If they answer yes, I ask them which newspaper(s) they read regularly. Those whose responses do not include China Times are in the experimental sample; those whose responses include China Times are not, and I call these participants existing consumers throughout the paper.

⁴I am cautious about using existing consumers as benchmarks to interpret the treatment effects because I do not measure their news consumption and so have no precise information on whether and how often they read *The China Times*.

⁵A screenshot of the email can be seen in Figure B.1

Figure 3.1: Overview of Experimental Design



The customized URLs not only ensure that people can access the website only through the hyperlink but also help me to identify each participant’s browsing activity on the website. In the following days, I send daily reminders to participants about the website. I disable the website link after the election and launch an endline survey the next day.

I note three points: first, I do not tell participants that they are randomly assigned to receive the website access; that is, they do not know their treatment conditions, thus no performance bias.⁶ Second, the experiment does not force participants to browse the website content but allows them to decide whether and how much they want to do so, which is more natural and closer to people’s media consumption behavior. Third, I choose to

⁶Guarding against “demand effects”—cues in the setting that suggest to experimental participants what is expected of them—is important for experiments (Mummolo and Peterson, 2019). To limit the impact of demand effects, I undertake two precautions. First, instead of telling participants that this study is about the pro-Beijing media, they are told that the study is about voters’ opinions about political and societal affairs. This description could discourage participants from wondering what I intended to do. Second, I do not inform participants that they are randomly assigned to receive website access, preventing such information from affecting the behavior and attitudes of interest.

present the pro-Beijing news in the form of online newspapers instead of print ones for the following reasons: In Taiwan, more people acquire news from the Internet over time.⁷ The Internet has become a major source of political news for citizens, second only to television.⁸ In addition, using a tailored website allows me to measure individuals' media exposure in a more precise and unobtrusive fashion; for example, I can measure whether (and how much) people are actually treated. Gerber, Karlan and Bergan (2009) distribute their treatment in the form of newspaper subscriptions, admitting that they cannot be sure that the newspapers were read after people received them. Although survey-based measures of media consumption are widely used in previous studies, they are plagued with questions about validity (Guess, 2015; Prior, 2013).⁹

The Pro-Beijing News Website Over the course of the experiment, I update the website on a daily basis with real-time news articles from *The China Times*. I standardize the way to choose news articles. First, I select all news stories from the front page of *The China Times*.¹⁰ Second, I select all China-related news stories from the cross-strait-relations page of *The China Times*.

When participants click the hyperlink provided in the emails, they are redirected to the website's homepage. Participants can click date icons on the homepage to access the news articles for a given publication date. For example, by clicking the date icon 2020-01-01, participants will enter a new webpage containing the news articles published on that date.

⁷The Taiwan Communication Survey, a national survey, asks respondents to report their frequency of acquiring online news on a 4-point scale (never, seldom, sometimes, and often). In the 2003 wave, 26.5% reported that they often or sometimes do so; in the latest 2015 wave, the number increases to 74.2%.

⁸In the latest 2015 Taiwan Communication Survey, 75.1% of respondents often or sometimes acquire political news from TV, and 65.2% of them do so on the Internet. See <http://www.crctaiwan.nctu.edu.tw/material/files/5358772016.pdf>.

⁹When benchmarking participants' actual exposure to the website against their self-reported exposure, they indeed tend to overreport their news consumption. I will discuss this finding in a later section.

¹⁰Newspaper readers are attracted to stories because of the content of the headlines or the placement of stories. Front-page stories are more likely to be read than articles buried near the back of the newspaper (Kahn and Kenney, 2002).

For the placebo website, I also update the website daily, selecting news articles from the entertainment pages. I ensure that the length of the news articles on the placebo website is similar to that on the treatment website.

An important choice in my design is to truthfully deliver the news source that produced each news article. I did this not only to avoid deception but also to promote external validity.

Figure B.2 is a screenshot of the website's front-page; I report the headline of each news article chosen from *The China Times* in Appendix Section A, followed by results from a text analysis of the news articles reported in Appendix Section B.

3.4.1 Browsing Behavior

I use Google Analytics to track participants' browsing behavior on the website. One advantage of using this web analytics service is that participants do not need to install any software to produce the web traffic data. Note that I do not track their external browsing activities. Results show that 50.05% of treatment group participants visit the site during the two weeks after they receive the hyperlink. Among these participants, around 93% of them return to the site (i.e., 6.67% visit the site only once). For those who have session recordings, 74.94% of them spend an average of one minute or more per day on the site, and 44.16% spend an average of three minutes or more on the site.¹¹ 88.5% of the visitors (i.e., visit at least once) complete the endline survey. Table 3.1 reports the mean and standard deviation of participants' browsing behavior (both treatment and placebo).

¹¹For the placebo group participants, 50.39% of them visit the website at least once. Among those who visit the website, 80.52% spend at least an average of one minute per day browsing the website, and 45.78% spend at least an average of three minutes per day. These numbers are similar to that of the treatment website.

Table 3.1: Browsing Behavior

	Treatment Group (N=941)		Placebo Group (N=377)	
	Mean	SD	Mean	SD
<i>Panel A. Among all participants (in percentage)</i>				
Visited the website at least once	50.05%	50.02%	50.39%	50.06%
Minimal Compliers	37.51%	48.44%	40.58%	49.17%
Full Compliers	22.10%	41.51%	23.07%	42.18%
<i>Panel B. Average browsing time (in minutes)</i>				
Visited the website at least once	3.42	3.5	3.38	3.47
Minimal Compliers	4.41	3.53	4.08	3.52
Full Compliers	6.14	3.68	6.05	3.72

Note: Panel A reports the statistic with respect to all participants in the treatment (placebo) group. Panel B reports average browsing time of participants. Minimal Compliers is a term referring to cases in which an average of one minute per day spent visiting the site is the threshold value to define compliers. Full Compliers refers to the case in which an average of three minutes per day is the threshold value to define compliers.

Figure B.3 reports the distribution of average browsing time among participants who visit the news website (both treatment and placebo). I also report the browsing activities across individual and time. In Figure B.4, a red (white) rectangle means that a participant accesses (does not access) the news site on that date. The figure excludes participants who never visit the site. Finally, I show that compliers seem distributed evenly among partisan lines (more details in a later section), implying that participants appear not to live in an echo chamber in terms of their online news consumption (see Dvir-Gvirsman, Tsfaty and Menchen-Trevino (2016); Garrett (2009); Guess (2020)).

3.4.2 Outcome Variables

I examine three outcome variables: vote decision, candidate evaluation, and opinion about China. These variables are measured in both survey waves. Below, I describe how these variables are measured and coded. Table B.2 in Appendix Section C reports summary statistics of the variables.

Vote decision I measure participants' baseline vote intent and realized vote choices in the election. In the baseline survey, participants are asked to name the presidential candidate

for whom they planned to vote (including the undecided option). I create a variable coded 1 for those whose answer is Han and 0 otherwise to indicate whether a participant intended to vote for China's preferred presidential candidate before the experiment. In the endline survey, I ask participants who report they voted in the election: "Who did you vote for the presidential candidate (including the void ballot option)?" I create another variable coded as 1 if participants report that they vote for Han in the election and 0 otherwise. By subtracting the baseline from the endline score, the variable "Change in Vote for Han" is the shift in vote for Han from the baseline to endline survey.

Candidate Evaluations The feeling thermometer, a standard measure, is used to gauge participants' candidate evaluations. I ask participants in both waves: "We would like to know how you feel about the presidential candidates in the 2020 Election. Please express your feelings on a scale of 1 to 10, where 1 means very unfavorable and 10 means very favorable." Participants are asked to rate each of the three candidates. In the main analysis, I focus on the comparative feelings toward Han and Tsai, the two main candidates. I subtract Tsai's score from Han's score so that higher scores represent more positive evaluations of Han. The variable "Change in Candidate Evaluation" is the shift in the Han minus Tsai feeling thermometer score from the baseline to endline survey (-18 to 18; positive values indicate Han became more favorable).

Opinions about China I assess participants' attitudes toward China-related issues by asking the following five questions in both waves. To mitigate the concern about multiple testing, I create an index by averaging survey responses to the five questions in each wave ($\alpha=0.73$ in baseline and 0.80 in endline). Before creating the index, I recode the responses in a way such that higher scores indicate more favorable to China. I use the index in the main analysis and report the separate regression results of each survey question in Appendix Section E. The variable "Change in Pro-China Index" is the shift in the index score

from the baseline to endline survey, with positive values indicating China became more favorable to participants.

Variables (scales)	Survey Questions
Favorability of China (1–10)	We would like to know how you feel about China. If 1 means very unfavorable and 10 means very favorable, what is your general feeling toward China? (1=very unfavorable; 10=very favorable)
China Threat (1–3)	Do you think China’s military is a major threat to Taiwan’s security, a minor threat, or not a threat? (1= Major threat, 2= Minor threat, 3=No threat)
Trade with China (1–5)	Some people claim that we should expand our economic relationship with China, but others advocate reducing that relationship. What do you think we should do? (1=Greatly reduce the economic relationship; 2= Somewhat reduce the economic relationship, 3= The current economic relationship is about right, 4= Somewhat expand the economic relationship, 5= Greatly expand the economic relationship)
Hong Kong Protest (1–5)	How much do you support the ongoing protest in Hong Kong? (1=Do not support at all; 2=Somewhat do not support; 3=Neutral; 4=Somewhat support; 5=Strongly support)
Radical Behavior (1–5)	To what extent do you agree with the following statement: When the Hong Kong government fails to listen, protesters are justified in using radical tactics. (1=Strongly disagree; 2=Disagree; 3=Neutral; 4=Agree; 5=Strongly agree)

Political Predispositions To test whether the pro-Beijing media has differential effects on participants with various political priors, I measure participants’ partisan identities in the baseline survey. They are asked: “Of the following political parties, which party do you usually support?” The answer of a participant who names a party is taken to be the party identification; if a participant does not answer unequivocally, then that participant is asked, “To which political party are you more inclined?” If the participant names a party, that answer is taken to be the party identification.

Following most existing research on Taiwanese politics (e.g., Tsai (2017)), I define pan-Blue participants as those who either support or lean toward the following parties: KMT, New Party, or People First Party. Pan-Green participants are defined as those who either support or lean to the following parties: DPP, Taiwan Solidarity Party, Green Party, or New Power Party. Independent participants, or nonpartisans, are those who neither support nor lean toward any parties, or those who support or lean to the following two parties: Taiwan People’s Party or Tree Party, whose positions on Beijing are ambiguous.

This operationalization well captures participants’ pretreatment opinions about China. Figure B.5 reports the mean scores of the five questions related to China by partisan identities, showing that pan-Blue participants have significantly more favorable attitudes toward China a priori, followed by independent and then pan-Green participants. I also regress people’s baseline scores of the China Index on their personal characteristics (age, educa-

tion, income level, residence location, and ethnic origin, and partisanship), finding that partisanship is the strongest predictor, at least four times as much as other demographic factors.

3.4.3 Logistical Details

Recruitment I commission Qualtrics to recruit Taiwanese citizens eligible to vote in the 2020 election (i.e., 20 years old and above) to participate in a two-wave online survey. People opt in to participate in both waves and can leave the surveys any time. The provision of the website and monetary rewards is never mentioned during the recruitment, assuaging concerns about sample selection based on participants' interests in news or financial incentives a priori.

The baseline survey adopts a quota sample based on age, gender, and partisanship. In particular, 30% of the experimental sample are pan-Blue participants, another 30% are pan-Green participants, and the remaining 40% are independents. The partisan distribution in the sample is consistent with that in the population. In the survey, I measure participants' baseline, pretreatment outcomes, and background information, including demographic characteristics, partisanship, past voting experience, political interest and engagement in the 2020 election, and media diet. The information serves as the basis for balance checks and the criteria for heterogeneity analysis.

I successfully recruit 2,077 participants who complete the baseline survey. A total of 195 of the participants are existing consumers of *The China Times*, and 1,882 of them are not.

Treatment Assignment After concluding this survey, I perform a blocked randomization based on partisanship for the treatment assignment. This practice in expectation can reduce sampling variability, as people in the same block are expected to have similar potential outcomes. It also ensures that a specific proportion of subgroups are available for heterogeneous analysis (Gerber and Green, 2012, 71-77).

I first exclude the 195 participants who are existing China Times consumers from the sample, then classify participants into one of the three partisan blocks (i.e., pan-Blue, pan-Green, and independent). In each block, I randomly assign half the participants to the treatment group, 30% to the control group, and 20% to the placebo group. Together, among the 1,882 experimental participants, 941 are in the treatment group, 564 in the control group, and 377 in the placebo group.

Endline Survey In the week following the election, I invite all participants to the endline survey and successfully recontacted 949 of them (recontact rate = 45.69%). A total of 861 of the 1,882 experimental participants in the baseline survey completed the endline survey (recontact rate = 45.74%), and 88 of the 195 nonexperimental participants in the baseline survey completed the endline survey (recontact rate = 45.12%). The attrition rate does not differ by treatment conditions (p -value = 0.614). The 949 participants constitute the main sample throughout this study.

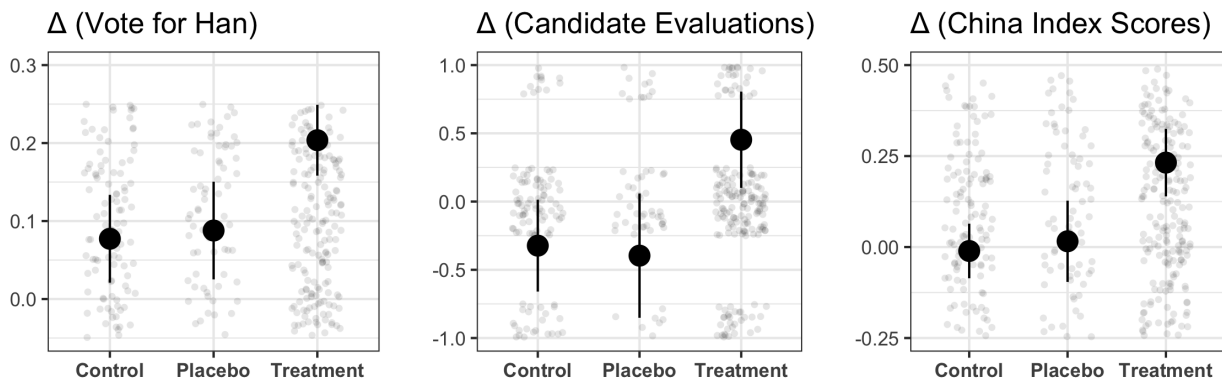
Attrition and Balance Check Table 3.2 reports t -tests for selective attrition and ANOVA tests for sample balance across treatment conditions. Overall, participants who have completed baseline and those who completed endline are statistically indistinguishable from each other in terms of demographic characteristics. In addition, participants from the three experimental conditions are not jointly different from each other across nearly all background characteristics, even after attrition (the exception is past voting behavior in 2016). Figure B.7 also confirms that the baseline outcome scores are balanced across conditions.

To further assuage the concern about sample selection bias, I also examine whether the interaction of background covariates and assignment to treatment predicts attrition. Figure B.8 shows no evidence that either treatment (i.e., political news website and entertainment news website) led to a severe sample selection bias in terms of the observable characteristics of individuals who responded to the endline survey.

3.5 Results

I first conduct descriptive analyses of the data. Figure 3.2 reports the means of outcome scores for each experimental condition.¹² Results reveal two patterns: (1) the treatment group is significantly different from both the control and placebo groups; (2) the control group and placebo group have no discernible difference across all three outcome variables. I use a one-way ANOVA to test the mean differences of these groups. For vote decisions, I reject the null hypothesis that the group means are identical [$F(2, 735) = 7.50, p=0.0006$]. For candidate evaluations, the null hypothesis that changes in candidate evaluations are indistinguishable across groups is also rejected [$F(2, 856) = 6.30, p=0.0019$]. Finally, experimental participants do not hold jointly identical opinions about China before and after the experiment [$F(2, 856) = 8.27, p=0.0003$]. The results suggest that the treatment had a direct influence on behavior and opinions.

Figure 3.2: Changes in Mean Outcome Scores by Experimental Conditions



Note: The left panel plots the means of individual-level changes in vote decisions in Han's favor ($N=738$). The middle panel is changes in candidate evaluations in Han's favor ($N=861$). The right panel is changes in mean scores of the China Index ($N=861$). The outcomes are change scores from the baseline to endline survey.

I examine the differences in a regression framework. For simplicity and greater statistical power, the following analysis pools the control group with placebo group together because

¹²Note that the outcome variables in the main analysis are change scores rather than endline scores; I show in Online Appendix that my results are identical to results using endline scores as the outcome and baseline scores as the control.

they do not differ in any outcome dimension (see Appendix Section D for results comparing control and placebo group). As I show below, the placebo group allows me to address multiple concerns about the results and to evaluate possible mechanisms underlying the treatment effect.

Both intent-to-treat (ITT) and treatment-on-the-treated (TOT) effects are estimated in this paper. ITT refers to the effects of treatment assignment on outcomes by comparing outcomes of participants assigned to treatment and control, regardless of whether participants are actually treated. In this study the ITT estimates refer to the effects of providing access to the pro-Beijing website, which average the effects on compliers and noncompliers. The TOT estimates, by contrast, refer to the effects on those who spend some time browsing the pro-Beijing news website. I expect that TOT effects are larger than ITT effects.

To estimate ITT, I regress outcome scores on treatment status indicator. For TOT effects, I use two-stage least squares estimand, widely used in field experiments following the approach of Angrist, Imbens and Rubin (1996). Because website access is randomly assigned, I use it to instrument participants' exposure to the news website (first stage) and then use the exogenous variation in exposure to estimate its effects on outcomes (second stage). The TOT estimates are unbiased when exclusion restriction and monotonicity are met.

For TOT, I use two different threshold values to distinguish compliers and noncompliers. I define participants who spend an average of three minutes or more browsing the website during the two-week window as compliers (Full Compliers). A less strict definition of compliers is that those who spend an average of one minute or more are considered compliers (Minimal Compliers).

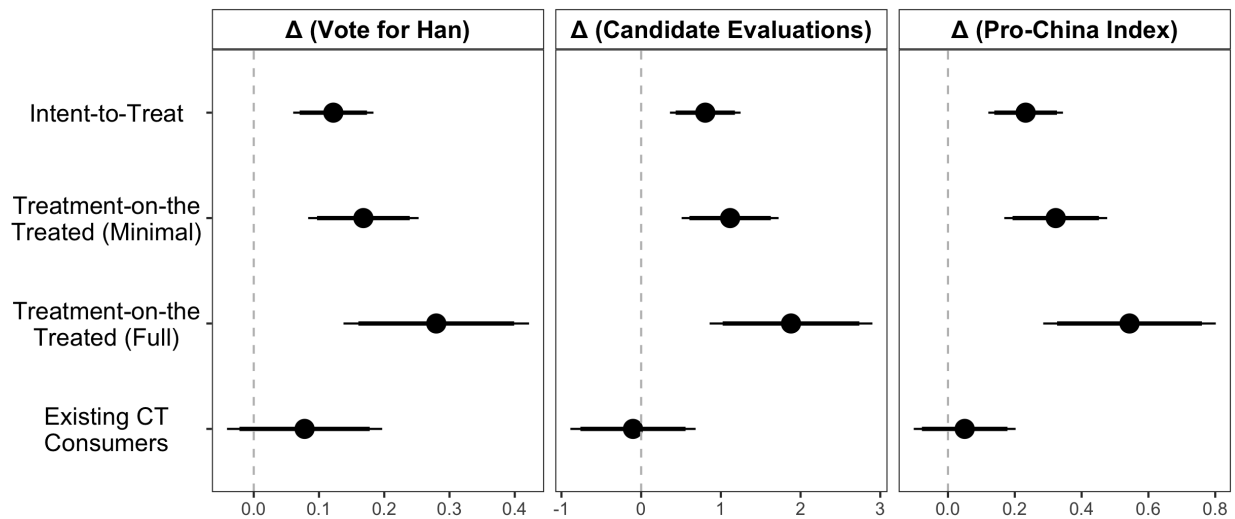
Become Prone to Vote for Beijing's Preferred Candidates Participants in the treatment group become more likely than those in the control group to vote for China's preferred presidential candidate Han (p-value = .001). The ITT effect is estimated between 0.06 and 0.183 for a mean change in Vote for Han of 0.144, on a scale coded between -1 and 1. This

represents a 0.28 standard deviation difference between the two groups, a nontrivial difference. I also find that the effect is significantly larger among those who have not made their vote decisions in the baseline.

I next turn to TOT effects. Both Table 3.3 and Figure 3.3 show that the estimated TOT seems larger than the estimated ITT. Comparing the coefficient sizes of the two complier cutoffs shows that the effects are more pronounced among those who spend more time on the website. Some evidence indicates that existing consumers become slightly willing to vote for Han in the election, but the estimate is not precise enough to reject the null hypothesis.

Appendix Section F provides evidence on how the pro-Beijing media moves votes. It shows that the pro-Beijing media mainly persuade those undecided on whom to vote for in the baseline survey to choose Han rather than deter voters from their vote intention.

Figure 3.3: Coefficient Plot: ITT, TOT, and Existing Consumers



Note: Regression estimates of intent-to-treat effects and treatment-on-the-treated effects with 95% and 90% confidence intervals, respectively. This plot reports the estimated treatment effects on vote decisions, candidate evaluations, and beliefs on China. ITT is estimated by comparing outcomes of the treatment group and control group (control + placebo participants). TOT is estimated by two-stage least squares regression, in which the treatment assignment indicator is used to instrument for compliers. The exogenous variation in compliers is then used to estimate the relationship between exposure to the pro-Beijing news (compliers) and the three outcome variables. Existing Consumers refer to those who already read *The China Times* regularly on a daily basis. The estimate for them is the mean differences of baseline and endline.

Change Candidate Evaluations in Han's Favor Evidence on candidate evaluations is consistent with the findings in vote decisions, which may not be a surprise in that people's feelings about candidates are often proximate to their vote choices. Table 3.3 and Figure 3.3 show that treatment group participants become more positive toward Han than those in the control group ($p\text{-value} = .001$). The ITT effect is estimated to be between 0.36 and 1.24 for a mean change in Candidate Evaluations of 0.056. This means that the two groups differ by 0.24 standard deviation. The estimated TOT is larger than the estimated ITT. By contrast no evidence shows that existing consumers become more favorable to Han during the study period. The coefficient is almost zero and has a wrong sign. The Online Appendix reports separate regression coefficients on each of the three presidential candidates' feeling thermometers. On average, the treatment had a positive effect on evaluation of Han, negative effect on Tsai, and indiscernible effect on Soong.

More favorable toward China The treatment group becomes more positive toward China in the endline than the control group does ($p\text{-value} = .001$). ITT is estimated to be between 0.12 and 0.34 for a mean change in the China Index of 0.11, which represents the difference in the two groups by a 0.27 standard deviation in the endline. TOT estimates are larger than ITT estimates. The comparison of TOT estimates using different complier cutoffs once again confirms that the positive effects are more noticeable among those more engaged in the treatment website. For the existing consumers, weak evidence shows that their opinions about China become more approving in the study period.

Appendix Section E reports separate regression results on the five China items, showing that the treatment group participants at the end of the experiment have more positive feelings toward China, become less likely to view China as a threat, become more eager to have trade with China, and become more negative to the 2019–2020 Hong Kong Protests than control group participants. The effect size of TOT seems larger than ITT.

One more issue can bolster the results. Around mid-December 2019, the Anti-Infiltration

Act became a central issue in the presidential election campaign. The DPP claims that the passage of the act is necessary to regulate the potential Chinese influences on Taiwan's domestic politics. The pro-Beijing media publishes several news articles against such a proposition (see Appendix Section A). I thus evaluate whether the opinions of participants in the treatment group and control group differ on this new act. I solicit supports for the act only in the endline because when the baseline survey was in the field, this act had not received media attention. This is also the reason I did not preregister this question. The question reads as follows:

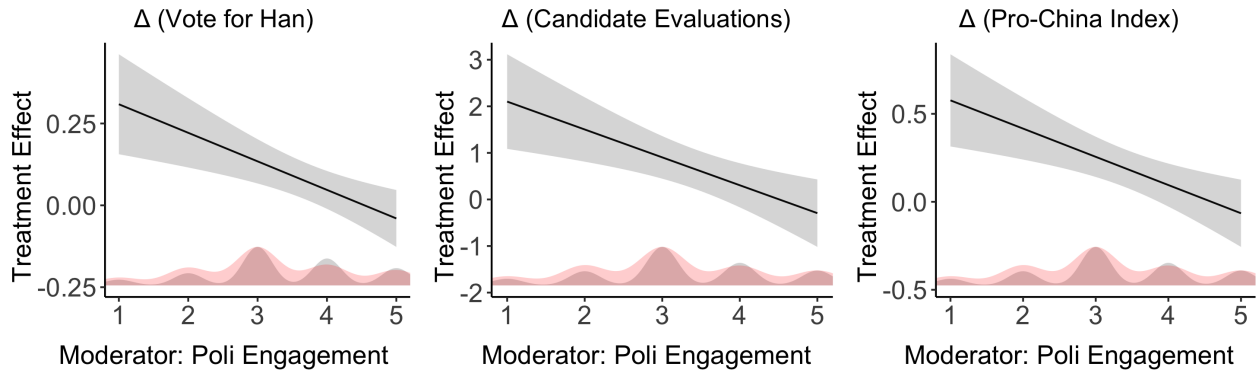
The anti-infiltration bill was passed by the legislature on Dec. 31, 2019. Some people think the bill would further hamper cross-strait exchanges, but others claim that the bill is a safety net for our democracy. We would like to ask how strongly you support the bill.

The variable is measured on a 5-point scale, ranging from "very supportive" (1) to "not supportive at all" (5). It has a mean of 3.004 and a standard deviation of 1.201. Results show that (not reported) the treatment group participants were less supportive of the Anti-Infiltration Act than the control group participants (p -value = .001). In terms of ITT, the two groups differ by a 0.226 standard deviation.

Conditional Treatment Effects At the outset I expected that the treatment would have stronger effects among those politically inattentive than among those who are attentive (H2). Figure 3.4 shows that the effects are indeed significantly greater among those who less care about the election outcome (or who are less interested in the election campaign) prior to the experiment. By contrast the effects are indistinguishable from zero among those who are already politically informed.¹³

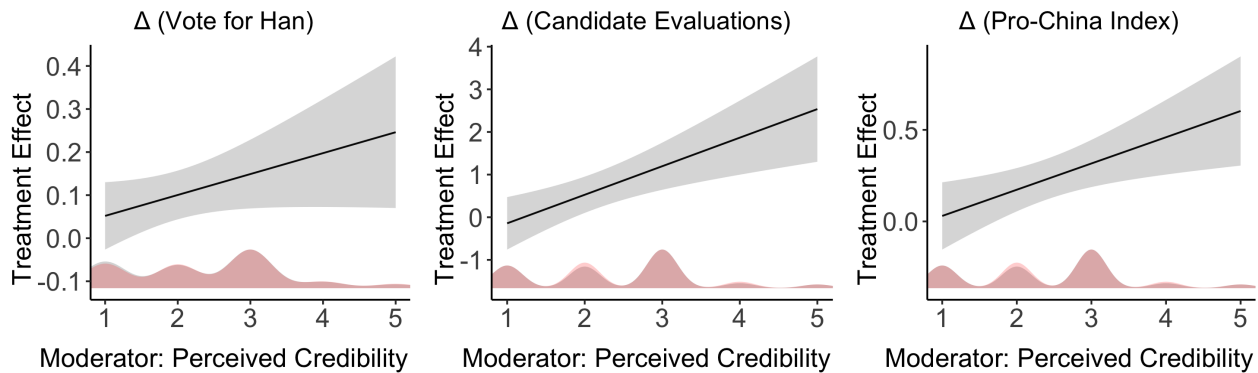
¹³I use several survey questions to measure people's political awareness or engagement, finding that the results are largely robust to all these different measures, including the frequency of sharing political news on social media, the frequency of acquiring news in the mass media, and the amount of time spent acquiring election news the previous week. See the wording of the survey item and results in Online Appendix.

Figure 3.4: Experimental Effects Conditional on Political Engagement



I also expected that the treatment would have stronger (weaker) effects among those who perceive (do not perceive) *The China Times* as a credible media outlet (H3). Respondents in the endline survey are asked how much they agree that *The China Times* is a red media. This variable is used as a proxy for participants' perceptions of *The China Times'* credibility. Figure 3.5 indicates that the treatment effects are weak and negligible among participants who view *The China Times* as a CCP-aligned media outlet. In contrast, the effects are stronger among those who disagree with this statement.

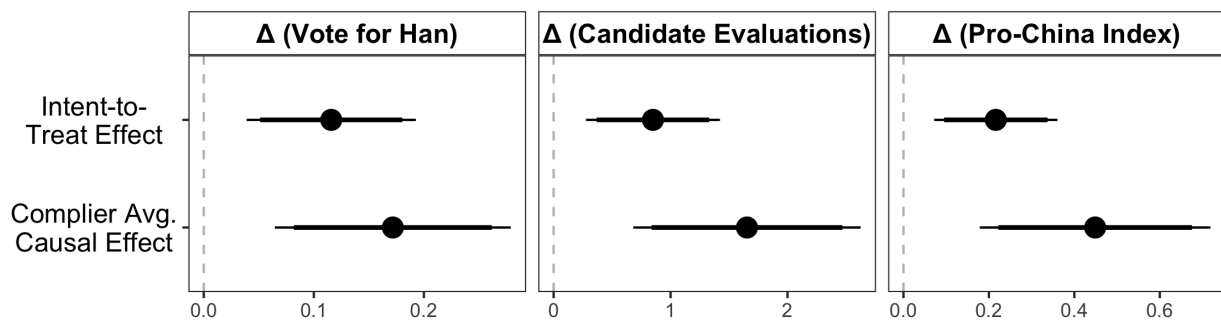
Figure 3.5: Experimental Effects Conditional on Perceived Credibility of *The China Times*



News Content or News Source? One may worry that the results are driven by news source rather than news content. The political news on the website includes information about the news source, so participants know the news articles are from *The China Times*; thus,

they may modify their beliefs based on the source cue. The placebo website helps me to address this concern. Because the source of the entertainment news is also from *The China Times*, significant differences in outcomes across the treatment group and the placebo group participants suggest that the news source, which holds constant, is not the driver of the results. I reanalyze the data by comparing outcomes of treatment group and placebo group, finding that the main results are still robust (see Figure 3.6). I also estimate the complier average causal effect, that is, effects among compliers.

Figure 3.6: Coefficient Plot: ITT and CACE (Treatment vs. Placebo Group)



Note: Estimated intent-to-treat effects and complier average causal effects, with 95% and 90% confidence intervals, respectively. I estimate ITT effect by comparing outcomes of the treatment and the placebo group (placebo group is the baseline group). I estimate CACE by comparing outcomes of the treatment and the placebo group, conditional on compliers. Compliers are defined as those who spend an average of three minutes per day during the experiment period. In mathematical terms, $CACE = E(Y1 - Y0 | \text{Complier}=1)$.

Misreporting Vote Choices? One may worry that participants hide their real vote choice in the survey. Even if I cannot know which people misreport their vote choice, I use a list experiment to test whether participants in the aggregate hide their choices. The list experiment is designed in the following way: all participants in the endline survey are randomly assigned to two groups and are provided with a list of things that they may have done. The list for the control group contains four baseline items (watched movie in a theater, bought Taiwan Lottery tickets, gave money to a charitable organization, and travelled to a foreign country). The treatment group receives a different list that includes the same baseline items plus a sensitive one: “After the election outcome came out this time, I hid my

true vote choice when asked whom I voted for.” All participants are then asked to tell how many of these things (instead of which thing) on the list they have done. Figure 3.10 shows that the two groups are indistinguishable in terms of the item counts (estimate = -0.030 [-0.188, 0.128]; N=949). I also perform a subgroup analysis of the pan-Blue participants, who may misreport support for the winner (Wright, 1993) but find no evidence that these participants on the aggregate level had been deceptive about their vote choices (estimate = -0.069 [-0.355, 0.217]; N=300).

3.6 Effect Heterogeneity

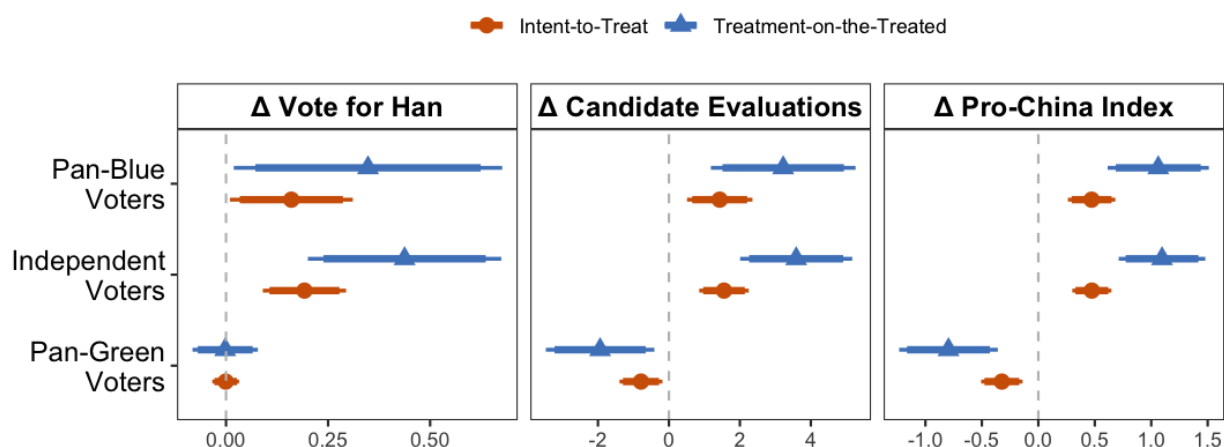
This section reports the results of subgroup analyses by partisanship to test my hypothesis that the pro-Beijing media has heterogeneous effects on people with opposing priors.

3.6.1 Heterogeneous Effects by Political Predispositions

Figure 3.7 reports both ITT and TOT estimates with their corresponding confidence intervals. Consistent with the effect heterogeneity hypothesis, the positive treatment effects occur in the pan-Blue and independent samples. The effects appear slightly stronger for Independents, which is consistent with the claim that nonpartisans are more influenced by additional information received. For pan-Blue participants, the treatment effects indicate that exposure to the pro-Beijing news can reinforce their preexisting beliefs.

By contrast a backfire effect is found in the pan-Green sample. The pro-Beijing media moves pan-Green participants’ feelings about Han and opinions about China downward. Both ITT and TOT estimates on pan-Green participants’ vote decisions are small and close to zero; this is likely because 80.6% of them already intended to vote for Tsai in the baseline survey so the glass ceiling limits the treatment effects. In contrast only 52.9% of pan-Blue participants in the baseline survey intended to choose Han in the election (See Appendix Section E and the conclusion section for more discussion). Appendix Section G reports the regression results of the subgroup analyses.

Figure 3.7: Heterogeneous Treatment Effects by Partisanship



Note: ITT and TOT estimates on outcome variable with corresponding 95% and 90% CIs, respectively. I conduct three subgroup analyses for each outcome variable. Pan-blue voters are those participants who report in the baseline survey that they either support or lean toward the following parties: KMT, New Party, Party First Party. Pan-green voters are those participants who report in the baseline that they either support or lean toward the following parties: DPP, Taiwan Solidarity Party, Green Party, New Power Party. Independent voters are those participants who claim that they are not inclined to any political parties or those inclined toward Taiwan People's Party or Tree Party. Estimates are drawn from people who complete both survey waves.

3.6.2 Why Heterogeneity?

I demonstrate that motivated political reasoning may account for the effect heterogeneity observed among partisans. If biased reasoning is the main mechanism driving the results, participants with opposing partisan attachments should exhibit differential cognitive and emotional reactions to the persuasive messages. In the endline survey, I ask a battery of questions measuring individuals' reactions to the news articles on the website to which they have access. I present the questions only to participants in the treatment and placebo groups who report that they browse the website that they were invited to visit.

Quality and Argument Strength The first set of questions asks participants to rate the overall quality and argument strength of the news articles that they read on the website.¹⁴

¹⁴For the Quality question, I ask participants: "What do you think of the overall quality of the news articles you read on the website?" We measure this question on a seven-point scale, where 1 refers to extremely poor quality and 7 refers to extremely good quality. For the question on Argument Strength, I ask participants:

These questions serve to test whether people evaluate like-minded arguments as stronger and more compelling than opposing arguments (Lodge and Taber, 2013). I expect that in the treatment group, pan-Blue participants have more positive evaluations of the quality and argument strength of the news articles than pan-Green participants.

Emotion and Reactance Emotion has received growing scholarly attention in the study of motivated political reasoning. Recent studies find that anger is nearly alone in motivating individuals to engage in selective information processing (Hasell and Weeks, 2016; Suhay and Erisen, 2018). People experience anger when their values (Mullen and Skitka, 2006) or political opinions (Redlawsk, 2002) are threatened. Irritation then causes people to lean on their prior beliefs and respond in a hostile manner toward ideas that undermine them (Lazarus, 1991).

I ask participants how the news articles they read on the website make them feel. I measure four emotions and use them to create two variables.¹⁵ Specifically, Positive Emotion is the average of the scores of participants' responses to two feelings items—hope and enthusiasm—related to their positive feelings to the news articles. Negative Emotion is the average scores of participants' responses to two feeling items — anger and disgust — related to their negative feelings to the news stories. I expect that pan-Blue participants feel more positive toward the pro-Beijing news than pan-Green participants.

Traditional psychologists have long found that when people feel someone or something is trying to take away their choices or limit the range of alternatives, they tend to exhibit “reactance” by adopting or strengthening a view contrary to what was intended (Brehm et al., 1966). I solicit participants' reactance scores using the following questions developed by Dillard and Shen (2005):

"Generally speaking, how weak or strong do you think the argument was in the news articles you read. Again, please use the 7-point scale."

¹⁵The question reads as follows: "Generally speaking, how do the news articles you read on the website make you feel? Please rate your feelings about the news articles on a scale from 1 (feel no emotion at all) to 7 (feel emotion very strongly)."

- The news reports threatened my freedom to choose candidates.
- The news reports tried to make a decision for me.
- The news reports tried to manipulate me.
- The news reports tried to pressure me.

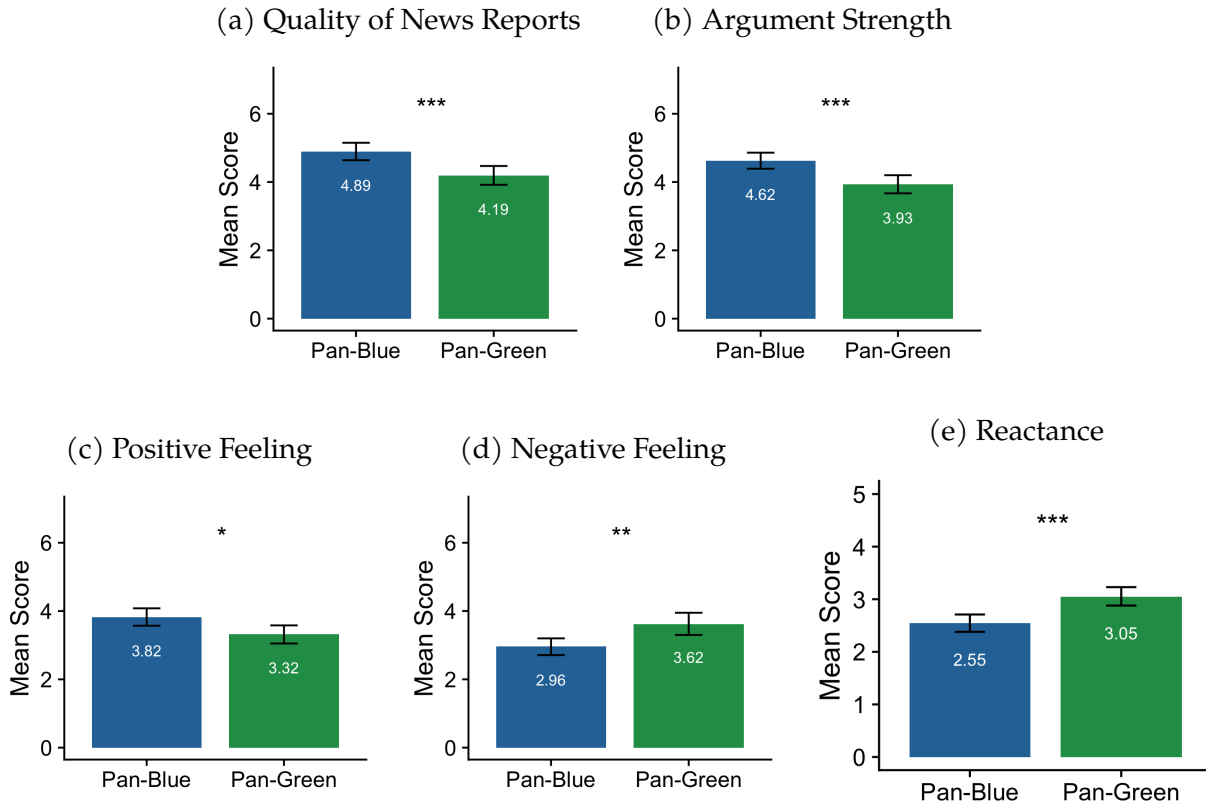
Participants are asked to use 1–5 to express the degree to which they agree with the statements, where 1 means strongly disagree and 5 means strongly agree. I create the variable Reactance by averaging their responses to the four questions, with higher values representing higher reactance. Among those in the treatment group, pan-Green participants should express higher reactance scores than pan-Blue participants.

Figure 3.8 reports mean scores with 95% CIs of participants' cognitive and affective reactions to pro-Beijing news articles. Results show that despite viewing the identical news, pan-Blue participants have more positive cognitive responses and positive affects to the political news than pan-Green participants. Both cognitive and emotional responses are "partisan," that is, in a direction consistent with how the treatment shifts the attitudes and behavior of the partisans in this study.

A Placebo Test One can argue that the differential cognitive and emotional reactions by partisanship are driven by news source rather than news content because participants may have known where *The China Times* stands relative to other media outlets. Cases can be made that pan-Green (pan-Blue) participants have more negative (positive) responses to the news not because the content of the slanted news is inconsistent (consistent) with their priors but because the news is from *The China Times* (See Baum and Gussin (2008) in the U.S. context).

To address this alternative explanation, I focus on the placebo group participants. If news source rather than news content is the driver of partisans' differential reactions, pan-

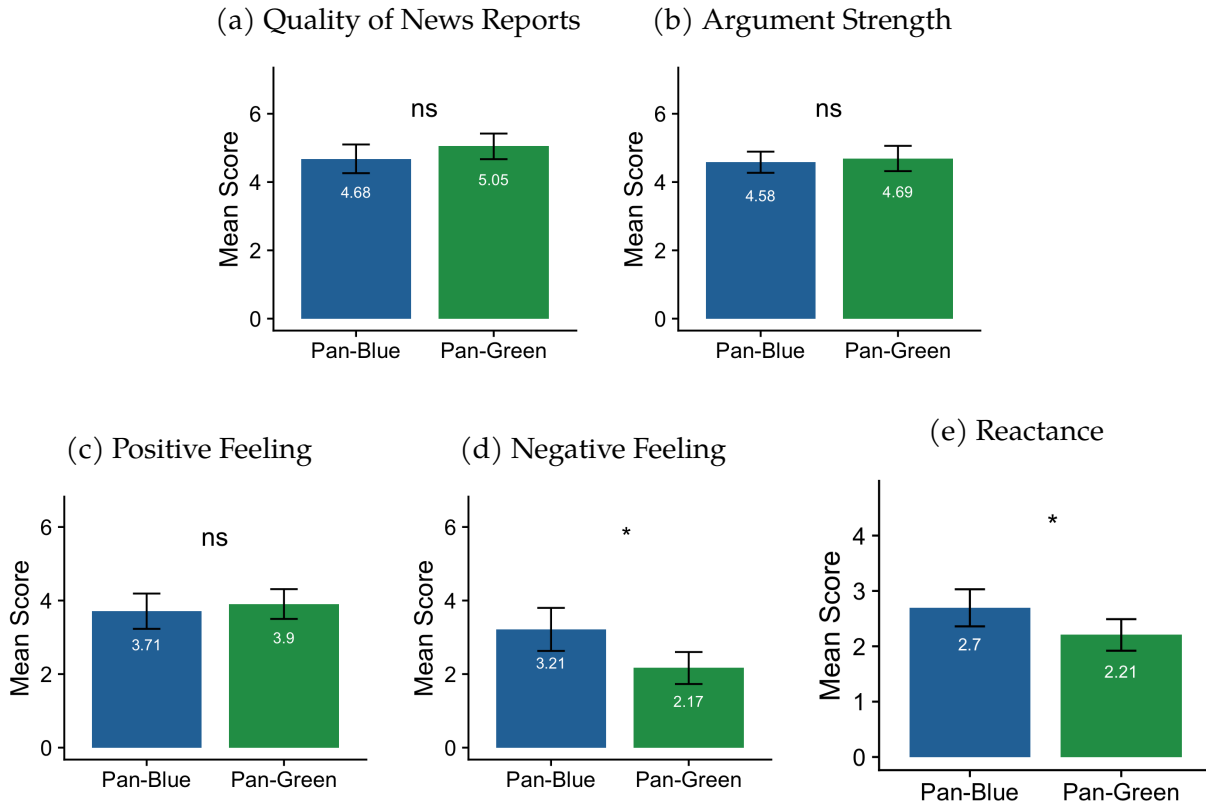
Figure 3.8: Cognitive and Affective Reactions to Political News



Note: These plots show the differences in mean scores between pan-Blue and pan-Green respondents exposed to the treatment website for their cognitive and emotional reactions to the news articles on the site. The bars indicate 95% CI. The plots also report the results of Wilcoxon test that compares group means. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$

Blue and pan-Green participants in the placebo group should also exhibit differential cognitive and emotional reactions to the news articles because the articles are also from *The China Times*. Figure 3.9 shows that this is not the case because the partisan pattern observed in the treatment group disappear in the placebo group. Partisans in the placebo group have similar cognitive reactions to the entertainment news, so do their affective reactions (except for negative emotions). Since the news source holds constant across groups, the lack of partisan reactions in the placebo group suggests that the news source does not trigger biased information processing.

Figure 3.9: Cognitive and Affective Reactions to Entertainment News



Note: These plots show the differences in mean scores between pan-Blue and pan-Green respondents exposed to the placebo website for their cognitive and emotional reactions to the news articles on the site. The bars indicate 95% CI. The plots also report the results of Wilcoxon test that compares group means. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, ns $p > 0.05$

3.7 Conclusion

This study offers the first field experiment that examines the impact of pro-Beijing media on voter behaviors and opinions during a presidential election. The evidence yields three major findings. First, the pro-Beijing media messages are capable of swaying Taiwanese voters, one of the most important targets by the Chinese government, in the intended direction. More generally, political context seems to play an important role in determining the effectiveness of China's information operations abroad. Second, the pro-Beijing media exert differential effects on voters with opposing political priors. Third, the effects seem mediated by voters' cognitive and emotional reactions to the political news received. In sum,

because Taiwan is not the only case that China intends to wield its influence using co-opted friendly media, what China does and how Taiwan reacts have ramifications for citizens in Hong Kong, Australia, and even the United States.

The research findings are a contribution to the existing literature and theorizing on the potential backfire effects of exposure to dissimilar political information. Contrary to some recent evidence, which finds exposure to opposing views to have no impact, and in line with other studies, my study shows that backfire effect is not the exception. Extending past work by incorporating people's evaluations of the accessed articles, the backfire effect found in this study seems to have its trace from people's cognitive and emotional reactions to the articles.

In addition, this study also makes a contribution to the growing body of work that uses behavioral data to study people's attitudes and behavior upon media exposure. Rather than relying on a forced exposure design, I incentivized exposure and accounted for compliance. Rather than asking participants to self report exposure, I tracked their actual exposure patterns on the news website. My research design combining systematic experimental treatment, online traces, and surveys pre- and post-experiment is most apt to accurately and precisely documenting the existence (or rather lack thereof) of pro-Beijing media effects.

People have more to learn about the effects of pro-Beijing media than can be revealed in a single experimental study. I hasten to note the scope of my evidence. First, the reader must be cautious in generalizing my findings to any broad inference because the experimental results may depend on several features of the 2020 Taiwanese presidential election. The election may be unique because Han is a nontraditional presidential candidate of the KMT. My survey data shows that many pan-Blue partisans seem less determined in this election to make up their minds. This would give this experiment more room to sway their candidate preferences and choices. Specifically, among the experimental participants, the odds of being undecided in the baseline survey for pan-Blue voters is 2.32 times that of pan-Green voters, holding personal characteristics constant. Further evidence of this is that among

those who selected the KMT presidential candidate in 2012, nearly 35% report that they are undecided and only 37.7% report they would vote for Han in the baseline survey. In stark contrast among those who voted for the DPP presidential candidate in 2012, only 15.8% are undecided and 75.4% decided to choose Tsai Ing-wen in the baseline survey.¹⁶

Second, more research should be conducted in different countries using field experiments to evaluate whether my findings are replicable in countries with similar political context. In the existing literature on media persuasion using a field experiment during elections, virtually all evidence is from the United States and Western European countries. More evidence beyond these countries is clearly needed.

¹⁶As to participants' (self-reported) voting behavior in 2012, apart from those who selected either the KMT or DPP presidential candidates discussed above in the main text, 82 participants say that they voted for the third-party candidate; 354 participants did not vote; 268 participants were under 20 in 2012 and ineligible to vote; and 253 participants no longer remember for whom they voted in 2012.

Table 3.2: Summary Statistics, Attrition, and Balance Tests

Variable	Sample and Attrition			Treatment Balance				
	Baseline Mean (SD) (1)	Endline Mean (SD) (2)	<i>t</i> -test p-value (3)	Existing Mean (SD) (4)	Control Mean (SD) (5)	Placebo Mean (SD) (6)	Treatment Mean (SD) (7)	ANOVA Test p-value (8)
Age	37.584 (11.460)	38.217 (11.133)	0.281	42.830 (12.780)	37.527 (10.890)	37.933 (11.277)	37.805 (10.684)	0.920
Female	0.495 (0.500)	0.498 (0.500)	0.619	0.375 (0.487)	0.519 (0.501)	0.476 (0.501)	0.519 (0.500)	0.602
Residence	0.821 (0.383)	0.821 (0.384)	0.846	0.875 (0.333)	0.827 (0.379)	0.835 (0.372)	0.801 (0.400)	0.530
Education	4.614 (1.032)	4.590 (1.027)	0.154	4.920 (0.937)	4.585 (1.046)	4.598 (1.078)	4.524 (1.003)	0.642
Full-Time Job	0.735 (0.441)	0.736 (0.441)	0.613	0.739 (0.442)	0.700 (0.459)	0.750 (0.434)	0.751 (0.433)	0.307
Married	0.483 (0.500)	0.484 (0.500)	0.579	0.636 (0.484)	0.458 (0.499)	0.494 (0.501)	0.465 (0.499)	0.751
Income	6.039 (3.257)	6.022 (3.150)	0.652	7.227 (3.132)	5.831 (3.166)	6.335 (2.912)	5.776 (3.175)	0.136
Pan-Blue	0.306 (0.461)	0.316 (0.465)	0.492	0.557 (0.500)	0.300 (0.459)	0.287 (0.454)	0.288 (0.454)	0.937
Independent	0.399 (0.490)	0.390 (0.488)	0.504	0.205 (0.406)	0.400 (0.491)	0.427 (0.496)	0.407 (0.492)	0.858
Pan Green	0.295 (0.456)	0.294 (0.456)	0.965	0.239 (0.429)	0.300 (0.459)	0.287 (0.454)	0.304 (0.461)	0.914
Vote in 2016	0.587 (0.493)	0.599 (0.490)	0.296	0.682 (0.468)	0.531 (0.500)	0.671 (0.471)	0.595 (0.491)	0.016
Vote in 2012	0.579 (0.494)	0.591 (0.492)	0.279	0.727 (0.448)	0.538 (0.499)	0.604 (0.491)	0.590 (0.492)	0.305
Ethnicity	0.647 (0.478)	0.653 (0.476)	0.690	0.625 (0.487)	0.669 (0.471)	0.701 (0.459)	0.632 (0.483)	0.242
Religion	0.204 (0.403)	0.209 (0.407)	0.475	0.125 (0.333)	0.223 (0.417)	0.268 (0.444)	0.195 (0.396)	0.143
Newspaper Preferences	0.450 (0.498)	0.446 (0.497)	0.741	0.330 (0.473)	0.458 (0.499)	0.433 (0.497)	0.467 (0.499)	0.760
TV Program Preferences	2.763 (0.995)	2.776 (0.993)	0.713	2.955 (1.060)	2.665 (1.002)	2.841 (0.978)	2.780 (0.976)	0.157
Control	0.300 (0.458)	0.302 (0.459)	0.842	0.000	1.000	0.000	0.000	———
Placebo	0.200 (0.400)	0.190 (0.393)	0.327	0.000	0.000	1.000	0.000	———
Treatment	0.500 (0.500)	0.508 (0.500)	0.548	0.000	0.000	0.000	1.000	———
Obs.	2077	949	———	88	260	164	437	———

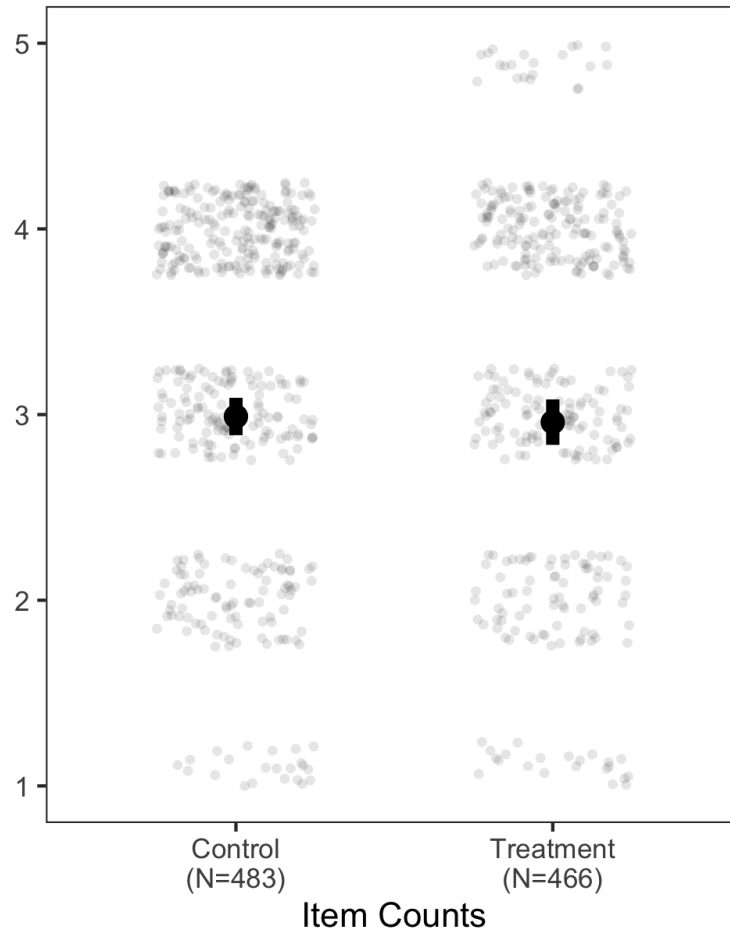
Note: Mean level of each characteristics are reported in column 1 for all respondents who completed the baseline survey (and corresponding standard deviation in parentheses). Column 2 for respondents who completed the endline survey (standard deviation in parentheses). For each characteristic, a *t*-test is conducted against the null hypothesis that respondents who have completed baseline and those who have completed endline do not differ from each other in terms of this characteristic. Column 3 reports the p-value for each test. Column 4 for endline participants who are China Times readers, column 5 for endline participants in the control group, column 6 for endline participants existing in the placebo group, and column 7 for endline participants in the treatment group. For each characteristic, an ANOVA test is conducted against the null hypothesis that participants in the control, placebo, and treatment groups do not jointly differ from each other in terms of this characteristic. Column 9 reports the corresponding p-value for each test.

Table 3.3: Treatment Effects on Behavior and Attitudes

VARIABLES	(1) Vote for Han	(2) Candidate Evaluation	(3) Pro-China Index
<i>Panel A: Reduced Form</i>			
Intent-to-treat	0.122*** (0.031)	0.805*** (0.226)	0.233*** (0.057)
<i>Panel B: Two-stage estimates</i>			
Treatment-on-the-treated (Minimal Complier)	0.168*** (0.043)	1.116*** (0.310)	0.323*** (0.079)
Treatment-on-the-treated (Full Complier)	0.280*** (0.072)	1.880*** (0.520)	0.543*** (0.132)
Observations	738	861	861
Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1			
<i>Panel C: Mean (SD)</i>			
Nonexisting consumers (experimental sample)	0.144 (0.431)	0.056 (3.351)	0.117 (0.848)
Control Group	0.077 (0.410)	(0.323) (2.755)	(0.010) (0.614)
Placebo Group	0.087 (0.385)	(0.396) (2.952)	0.015 (0.723)
Treatment Group	0.203 (0.452)	0.453 (3.750)	0.232 (0.986)
Existing consumers	0.078 (0.532)	(0.102) (3.760)	0.050 (0.729)

Note: This table reports regression coefficients with robust standard errors on outcomes variables. Column 1 for changes in vote for Han. Column 2 for changes in candidate evaluations. Column 3 for changes in the China Index scores. The index is generated by averaging the scores of five variables related to opinions about China. I record all variables such that higher values mean China's preferred behavior and attitudes. Panel A shows ITT estimates, where I regress outcome variables on the treatment status indicators. Panel B shows TOT estimates via two-stage regressions, where I use the treatment assignment indicator to estimate a first stage regarding whether participants are compliers. Panel C shows the means and standard deviations of the three outcome variables for the experimental sample and the nonexperimental sample. All statistics in the table are based on participants who complete both surveys.

Figure 3.10: List Experiment Embedded in the Endline (N=949)



Note: The experiment evaluates whether my participants provide deceived self-reports of their real vote decision at the aggregate level. The dots represent point estimates and bars the corresponding confidence interval at .05 level. The figure plots the model and data (distribution of observations) together.

Chapter 4

Persuasive Backfiring: A Survey Experiment on Citizen Compliance in China

4.1 Introduction

The centrality of taxation in state-building processes of developing countries receives growing attention (Brautigam, Fjeldstad and Moore, 2008). The demand for healthcare, educational opportunities, and other essential public services presses governments to urge taxpayers not to evade or underdeclare their tax obligations. The task of ensuring taxpayers to comply with government demand for taxation is as challenging as it is fundamental. According to an article in China Briefing News (2014), tax evasion has cost the Chinese economy an annual loss of 134 billion USD in tax revenues. Business elites especially tend to find ways, such as using bogus invoices and minimizing the paper trail to hide their revenue, to circumvent tax payments. A survey conducted by China's National Audit Office in 2004 shows that 788 large firms previously regarded as having sound accounting systems avoided paying 11% of their taxes over 2 years, equating to 25 billion yuan (Li and Ma, 2015, p.1). The Chinese government has consistently announced that it will get tough on tax evasion (China Daily, 2002; Wall Street Journal, 2016).

Although many possible factors could account for tax noncompliance in China, interview evidence suggests that many firm managers evade taxes because they think that the tax funds collected are not spent on projects that benefit the public and that government officials do not provide adequate public goods for their businesses to thrive. In one of my interviews, a firm manager notes that "if there is no way to buy a good government, why do I bother spending so much money on taxes?" This view aligns with the social contract theory, which contends that reciprocal relations between state and society drive individuals'

willingness to pay taxes. In this theory, people equate tax payments with signing a social contract with governments, which are expected to supply goods and services deemed important by their constituents in exchange for tax revenues. Tax compliance to a great extent hinges on individuals' perceptions of government responsiveness and representation. However, prior studies of this topic focus predominantly on developed democracies and conclude that "it is difficult to think of a psychological tax contract in autocratic regimes" (Feld and Frey, 2007, p.107). Therefore, our understanding of whether such social contract thesis exists in authoritarian countries like China is still limited.

A recent political change in China offers a valuable opportunity to examine whether people's tax compliance depends on their perceptions of government responsiveness. Scholars have reported a new pattern of congressional responsiveness unfolding in local China, showing that congress delegates are increasingly responsive to personal contacts and advocate local governments to deliver local public services (Manion, 2016). I exploit this political change to investigate the impact of individuals' beliefs in government responsiveness on their tax compliance.

A major empirical challenge in identifying such impact is that both perceptions of government and willingness to pay taxes could be driven by observable and unobservable factors. To overcome this issue, I conduct an original survey experiment that randomly assigns prospective business elites to receive information about increased congressional representation in local China. This political information is designed to update people's perceptions of government responsiveness, helping me credibly estimate the causal impact of exposure to the information on individuals' compliance attitudes.

Results show that the informational treatment significantly swayed people's willingness to pay taxes but in an unintended direction: Respondents who were prompted by the information on average exhibited lower tax compliance. Two pieces of evidence indicate that the results are not a statistical artifact. First, a manipulation check shows that the information updated beliefs in congressional responsiveness downward. Treatment group respondents

on average had a lower level of beliefs than control group respondents that local congress delegates are willing to respond to citizen demand. Second, the negative treatment effects still hold when using different compliance measures and model specifications. Together, I show that individuals' beliefs about government responsiveness could influence their tax compliance, suggesting that the psychological social contract may exist in both democracies and autocracies.

The backfire effect seems puzzling at first glance because the political information provided in this study was a positive message about local congresses. A more in-depth examination, however, reveals that a large proportion of study participants viewed the message as state propaganda. Many respondents linked the political information with government's attempt at propagating a political reality that is not really the case. They did not believe the persuasive argument and thought it is not consistent with what they saw and experienced. In line with Taber and Lodge (2006) and Redlawsk (2002), my study participants exposed to the informational treatment thus reported opinions more extreme than they otherwise would do. Since the prospective business elites in the study seem to possess a strong prior belief that local Chinese officials tend not to deliver sufficient public goods and services, as my interviews suggest, they may dismiss and even counterargue the information received. Consequently, the informational treatment resulted in the opposite of the intended effect, diminishing people's tax compliance.

My findings have important implications for three different lines of the literature. The first is political economy of taxation. I show that in authoritarian countries like China, people's perceptions of political representation and accountability could affect their tax compliance. As in democracies, the reciprocal relations between state and society in authoritarian China matter for people's willingness to follow their tax obligations. In contrast to previous work, I find empirical support for the fiscal social contract in the authoritarian context. The second is political communication in the form of persuasion. My findings are consistent with Huang (2018) and Bush et al. (2016) that hard propaganda often has a limited,

even backfire, impact on behavior and attitudes of people exposed to it. More broadly, I challenge the contention that political persuasion rarely backfires even under the most desirable condition (Guess and Coppock, 2018). The third is congressional politics in China. In contrast to extant research that considers congressional representation as the outcome variable (Manion, 2016; Truex, 2016), I employ it as the key explanatory variable. My findings suggest that even if Chinese local congresses have become more responsive, the way in which the ruling authorities communicate the political change with their citizens plays an important role in determining its political consequences.

4.2 Why Tax Compliance?

Existing literature has ascribed tax compliance to deterrence and morale. For deterrence, Allingham and Sandmo (1972) apply the Becker (1968) model of economic crime to show that individuals' compliance decisions depend on the expected utility of tax evasion, which is influenced by tax rates, odds of detection, and penalty to be paid if found guilty. Scholars later show, however, that the deterrence theory weights economic factors so much that it fails to explain why the actual compliance levels are often higher than predicted (Alm, McClelland and Schulze, 1992, 1999; Graetz and Wilde, 1985; Slemrod, Blumenthal and Christian, 2001). Scholars are thus prompted to find nondeterrence factors that fall outside the expected utility model but could affect tax compliance — tax morale.

Government's ability to collect taxes also depends on people's willingness to pay them. Tax morale is widely used as an umbrella concept to describe people's sociopsychological motivations to comply with tax regulations (Luttmer and Singhal, 2014).¹ Scholars have proposed four main morale-related factors that could influence tax compliance. The first is social norms, such as a feeling of pride, a sense of civic duties, altruism toward others, and an inclination to avoid feeling guilt and shame. In examining the impact of social norms on

¹Hereafter, I use tax morale, willingness to pay taxes, and tax compliance attitudes interchangeably.

compliance, scholars randomly send letters or emails containing moral suasion messages to test whether they promote people's tax compliance attitudes and behavior (Blumenthal et al., 2001; Torgler, 2004). The second is related to peer effects (i.e., how others pay taxes). A study in the United Kingdom finds evidence to support this claim (Hallsworth et al., 2017), but studies in Australia (Fellner, Sausgruber and Traxler, 2013) and in Peru (Carpio, 2013) find null effects. The third is cultural traits: observed differences in tax compliance levels persists over time and may be explained by different cultures across countries (Cummings et al., 2009; Halla, 2012).

The fourth, which is the interest of this paper, is the so-called fiscal social contract: citizens view tax payments as signing a contract with governments in return for public goods and services. Tax compliance is therefore motivated by people's attitudes toward governments, such as whether they feel active in the decision-making process (Feld and Tyran, 2002; Lü and Tsai, 2016), whether they perceive the state as legitimate (Levi, 1989), whether they have positive opinions about governing institutions (Torgler, 2005), and whether they feel the tax authorities fairly treat them (Torgler, 2007). Together, all these studies suggest that tax compliance embodies a reciprocal relation between governments and the public.

To date, empirical evidence on the social contract thesis is mixed. Blumenthal et al. (2001) randomly send out letters with the breakdown of government spending on social programs. Castro and Scartascini (2015) randomly inform taxpayers of specific public goods in their community. Dwenger et al. (2016) randomly send out letters containing the information about local German church tax revenues. All of them find null effects on taxpayers. By contrast, Hallsworth et al. (2017) find evidence that taxpayers are more compliant with the tax law when receiving information highlighting the breakdown of public expenditures. Lü and Tsai (2016) also find that Chinese citizens are more willing to pay property taxes when they have a chance to participate in the design of new tax laws.

In this paper, I overcome three issues often encountered in existing literature on tax compliance. First, I adopt an experimental method to avoid the endogeneity concern in-

herited in standard survey studies, such as Daude, Gutiérrez and Sánchez (2013). Second, the subject pool of previous lab and survey experiments mostly comprises college students, who tend to have no experience in paying taxes. By contrast, my study participants have had at least two-years' full-time working experience and are real taxpayers. More importantly, because of their college training and working experience, my study participants are arguably prospective business elites, whose tax compliance attitudes and behaviors are crucially important to the Chinese government. Third, most extant studies focus primarily on European, Latin American, and African countries but little on Asian (let alone authoritarian) countries.² This paper fills in the gap by fielding a survey experiment in China with prospective business elites to study whether taxpayers modify compliance levels as a result of exposure to information about enhanced congressional representation.

4.3 Theoretical Expectations

Fiscal Social Contract in Taxation The first theoretical building block of this paper is social contract theory, which posits that individuals consider paying taxes as an establishment of a contractual relationship with governments. As a result, people who perceive that governments sufficiently represent and respond to citizen demand are expected to be more willing to pay taxes. This theory has deep roots in early modern Europe, where monarchs were compelled to relinquish some of their power to legislative institutions in exchange for the ability to raise new taxes (Moore, 2004).

As Tilly (1992) noted, representative institutions in the case of England were the outcome of bargaining with the public for the wherewithal of state activity, especially the means of war. Kings of England initially did not form a representative institution but gradually conceded to barons, clergy, gentry, and the bourgeoisie to raise more tax revenues for warfare. This is where the notion *no taxation without representation* comes from. In addition,

²Some exceptions are Lü and Tsai (2016); Kao, Lü and Queralt (2021)

the reciprocal relations between state and society are also seen in the resource curse literature: Dictators circumvent pressures to implement representative institutions because they finance themselves with nontax revenues from natural resources such as petroleum (Ross, 2001).

This contractual relationship between state and society also involves public goods and provisions. Levi (1989) argues that citizens pay taxes largely because they perceive delivery of public services to the public by the government in return. To make the link between tax payments and public provisions clearer, governments also establish representative institutions (e.g., legislature) to lower the compliance cost. Timmons (2005) finds that countries with a high proportion of revenues from personal income and business taxes have better property protection rights, but countries relying more on regressive taxes have higher social spending.

In sum, according to the social contract theory, to encourage individuals' tax compliance, taxpayers need to be informed and persuaded that governments are responsive to citizen interests. Taxpayers' perceptions of whether government officials deliver sufficient public goods and services will subsequently affect their willingness to pay. This theory is psychological in nature.

Political Representation in China? Can the social contract theory travel to authoritarian countries like China? Conventional wisdom holds democracy to be a necessary condition for meaningful representation and accountability. That some scholars argue that "it will be difficult to think of a psychological tax contract in autocratic regimes" (Feld and Frey, 2007, p.107) comes as no surprise. Even if legislatures are increasingly common in contemporary authoritarian states and many of them are far from window dressing (Gandhi and Lust-Okar, 2009), the Chinese case seems still unpromising because congressmen have long been disparaged as "rubber stamps."

But researchers argue that Chinese congresses are undergoing a substantial change in

representation and responsiveness. Truex (2016) finds that provincial congress delegates have become more willing to address issues deemed most serious by the constituents they claim to represent. For nonpolitical issues, congress delegates have proposed policies that meaningfully reflect constituent interests; these proposals sometimes embody public provisions. One reason the Chinese government intends to improve congressional responsiveness is for information acquisition. Since Chinese leaders have limited information about citizen interests, they purposefully increase congressional responsiveness to understand mass preferences better and to placate citizens by providing a minimal standard of welfare (Truex, 2016, p.5).

Manion (2016) also finds that local congresses (municipal, county, and township level) have incrementally become more responsive since the Chinese government passed a new election law in the late 1990s that legalized direct elections of local congresses. Because current local congress delegates are nominated and elected by citizens, they have stronger incentive to be responsive. Conducting a large number of interviews and a survey with 5,000 local congress delegates, Manion shows that these delegates frequently use such terms as voting districts, constituency, and constituent interests. Moreover, most of them view themselves as representatives and thus side with their geographic constituents. In practice, local delegates have become more likely than before to take an initiative to advocate the local government for public goods and services, such as road building, irrigation system, environmental protection, and social order maintenance. This political change occurred in local China provides a unique opportunity to examine whether individuals' perceptions of government responsiveness prompted by the information about this political change affect their tax compliance.

Backfire Effects of Exposure to Dissimilar Information The second theoretical building block derives from research on public opinion and information processing. Political communication scholars have proposed the potential backfire effects of exposure to preference-

incongruent information (Taber and Lodge, 2006; Redlawsk, 2002; Nyhan and Reifler, 2010). Scholars interpret backfire effects as a possible result of the process by which people counterargue dissimilar information and bolster their preexisting views. If people counterargue unwelcome information vigorously enough, they may ultimately adopt opinions and behaviors in opposition to the direction of the information. In authoritarian countries, recent scholars find that hard propaganda often backfire because heavy-handed persuasive messages heighten people's awareness of the absurdity of the regime and the plight of the country (Huang, 2018). Because many participants in my study seem to exhibit strongly negative reactions to the information I provided about China's congressional representation, the information may produce a backfire effect on citizen compliance.

Hypothesis Based on the aforementioned theoretical perspectives, I hypothesize that the information about a recent increase in government responsiveness in China has a negative impact on people's tax compliance attitudes. Since the political information in the study contains a persuasive argument that seems widely considered as government's propaganda efforts, people exposed to the information would negatively process it, exerting a backfire effect on their tax compliance. The main hypothesis of this study is that individuals exposed to information about China's improved congressional representation will have lower tax compliance than those who are not exposed to the information (H1).

In this paper, I also examine two sources of effect heterogeneity. First, it is possible that people differ in their abilities to resist persuasive messages (Nyhan and Reifler, 2010). Defensive processing of persuasive messages is most likely to occur among individuals with better political knowledge because they are more adept at challenging argument-based persuasion. I thus hypothesize that the negative treatment effect is stronger among politically sophisticated respondents (H2).

The other source of effect heterogeneity considered in this study involves people's self-monitoring propensity. Gangestad and Snyder (2000) argue that individuals differ in the

degree to which they monitor and regulate their interpersonal behavior in response to social context. Specifically, high self-monitors are more likely than low self-monitors to carefully take social cues by tailoring their responses to fit prevailing social expectations. That is, low self-monitors are more likely than higher self-monitors to reveal their actual attitudes toward questions with socially desirable answers (e.g., tax compliance). I thus hypothesize that the negative treatment effect is stronger among low self-monitoring respondents (H3).

4.4 Empirical Strategy

I adopt a survey experiment of Chinese prospective business elites.³ Random assignment of individuals to receive a treatment averts the issue of self-selection into the treatment by balancing out the potential group difference in pretreatment covariates. With the stable unit treatment value assumption (SUTVA), the average difference in outcomes between treatment and control groups could be attributed to the treatment. This section includes discussion of my informational treatment, pilot study, logistical details, measures of outcome variables, and the moderating variables used to test effect heterogeneity.

4.4.1 Experimental Treatment

The informational treatment is a text-based vignette about China's increased congressional responsiveness. The vignette contains two pieces of information. The first is that local congress delegates have become more willing to respond to citizen interests and have played a greater role in public provisions than before. The second is the incentive of local congress delegates in responding to citizen interests. In short, the text-based vignette conveys *what* local congress delegates are doing and *why* they are doing so. The English translation of the vignette reads as follows:

³This study obtained approval from the IRB of the University of Texas at Austin under protocol 2016-09-0314.

Treatment Group

People's Congresses are a fundamental government institution in China. Local congress delegates meet at least once each year to discuss public policies. A recent survey of 5,000 local delegates shows that delegates have become more willing to respond to local people's interests and take the initiative to advocate for local governments to offer public services deemed important by people, such as roads, irrigation systems, and maintaining order. This differs from an earlier time when congress delegates seldom delivered public services. But now since township and county delegates are nominated and elected, they need to respond to people to gain a reputation. Yangzhou Municipal and Taizhou Wenling congresses are two specific examples.

Control Group

People's Congresses are a fundamental government institution in China. Local congress delegates meet at least once each year to discuss public policies.

Only respondents in the treatment group received the full vignette; by contrast, those in the control group received only the first two sentences in the vignette. I thus ensure that the only difference between the treatment and control group respondents is whether or not people receive the information about congressional responsiveness.

4.4.2 Pilot Study

In formulating the vignette, I conducted a focus-group discussion and a pilot survey prior to the main experiment. I had three virtual meetings to discuss congressional politics with four graduate students and a professor from a major university in Shanghai (University A) in late September 2016. These meetings facilitated the formulation of the content of the vignette. On November 3, 2016, I launched a pretest of 45 Chinese respondents. This pretest was designed to understand how Chinese people react to the information contained in the vignette. I asked respondents to provide feedback via three open-ended questions. Although most of them easily grasped the idea that the message they read is about congressional responsiveness, they generally did not believe the information. Many explicitly indicated that the political message read like state propaganda and questioned the accuracy

of the information. I provide more details in the Discussion section.

4.4.3 Logistical Details

This experiment consists of 119 Chinese prospective business elites with full-time work experience. I started to recruit respondents from a master of business administration (MBA) program at another major university in Shanghai (University B) on November 12, 2016. Two days later, I recruited more respondents from the MBA program at University A. In total, I successfully recruited 64 MBA students. On November 15, my Chinese partners exploited their social networks in China to recruit more respondents. I sent out a survey link to several Alumni Group Chats on WeChat (*weixin*) and incentivized them to do the survey through a feature of WeChat called "grab the red package" (*qianghongbao*); as a result 55 respondents completed the survey. All members in the Alumni Group Chats are university alumni with a major either in business administration or economics.

Compared to most extant tax experiments that recruit undergraduate students as the subjects, respondents in this study are more suitable to study tax-related issues. For MBA-based respondents, the admitted students must have full-time work experience for at least two years. Using University A as an example, the majority of the respondents have had more than a four- to five-year work experience. For those non-MBA respondents in the experiment, I use WeChat to prevent people who are still undergraduates from taking the survey.

For the experimental flow, respondents began the survey by answering a set of demographic questions (gender, age, and occupation). They were then randomly assigned to one of two groups: treatment and control groups.⁴ After reading the vignette, I asked respondents to answer the following question: *To what extent do you think local congress delegates are willing to respond to ordinary people's interests?* This question was designed as a manipula-

⁴I employ block random assignment to enhance statistical power. I divide respondents into three groups (two groups for two MBA classes and one group for university alumni) and then use a complete random assignment in each block.

tion check. Next, I asked respondents a battery of posttreatment questions about attitudes toward tax compliance (I discuss these questions in the next section) as well as their political knowledge, propensity for self-monitoring, and another set of demographic questions (party affiliation, monthly income, and residence location) before they exited the survey.

4.4.4 Tax Compliance, Political Sophistication, and Self-Monitoring

The outcome variable is individuals' attitudes toward tax compliance (i.e., their tax morale). In choosing the survey instruments, I follow Internal Revenue Service, World Value Survey, and Afrobarometer, all of which have been widely used in previous studies on tax compliance (Ali, Fjeldstad and Sjørnsen, 2014; Cummings et al., 2009; Halla, 2012; Daude, Gutiérrez and Sánchez, 2013). I use the three survey questions:

1. **Accountable:** how much do you agree that everyone who cheats on his or her taxes should be held accountable?
2. **Reasonable:** how much do you agree that people should just need to pay what they feel is a fair amount of taxes to government?
3. **Acceptable:** which statement do you agree the most?
 - Tax fraud is unacceptable under any circumstance because it is a matter of principle and fairness
 - Fraud and taxes are inseparable, as everyone evades taxes to some extent, and this is how the system is sustainable?

The first two variables—Accountable and Reasonable—are measured on a 7-point Likert scale, and the third variable—Acceptable—is a binary variable. I then create an index—Tax Morale—by averaging the standardized values of the three outcome measures. In the empirical analysis, this index is the main outcome variable. I also report the results using the three items separately for transparency.

For political sophistication, I propose six questions (see Supporting Information) to measure one's political knowledge; I then add them up and dichotomize the result by the mean to create a binary variable differentiating high political knowledge (1) from low (0). Zaller (1992) argues that the best measure of political awareness is tests of neutral factual information about domestic and international issues. For self-monitoring propensity, I use six items (see Supporting Information) proposed by Berinsky and Lavine (2011). The six items are classified into two categories: Positive-keyed items indicate that in a traditional true-false response format, a response of true is indicative of high self-monitoring propensity. *Negative-keyed items* suggest that a response of false refers to a high self-monitoring propensity. Based on the survey responses, I generate a binary variable dichotomized by the mean to indicate whether a respondent is a low self-monitor (1) or high self-monitor (0).

Table 4.1 reports summary statistics of all relevant variables, including respondents' background characteristics, outcome scores, and political sophistication and self-monitoring propensity.

4.5 Results

Randomization and Manipulation Check The treatment assignment worked properly. Results from t-test of two independent samples with unequal variance show that all pre-treatment covariates measured in the survey are balanced across groups. I also examine the mean differences for MBA and non-MBA respondents, respectively, and the results are identical. Table 4.2 reports results from three logistic regression models that regress people's treatment condition on their pretreatment covariates. In this table, Model 1 is for MBA respondents, Model 2 is for non-MBA respondents, and Model 3 is for pooled data. As expected, no pretreatment covariate predicts respondents' treatment condition at the .10 significance level. Followed by the balance check, I conduct a manipulation check to evaluate whether the informational treatment works. Remember that the treatment was arguably

Table 4.1: Summary statistics

Variable	Mean	Std. Dev.	Min.	Max.
Gender (Female=1)	0.513	0.502	0	1
Age	29.387	4.842	22	51
CCP Membership	0.387	0.489	0	1
Income Category	6.353	2.276	1	10
Marital Status (Married=1)	0.378	0.487	0	1
MBA Program (Yes=1)	0.538	0.501	0	1
Manipulation Check	3.118	0.845	1	5
Tax Morale	0	0.636	-1.6	1.012
Accountable (Standardized)	0	1	-2.449	0.96
Accountable (Unstandardized)	5.311	1.76	1	7
Reasonable (Standardized)	0	1	-1.687	1.273
Reasonable (Unstandardized)	4.42	2.027	1	7
Acceptable (Standardized)	0	1	-1.232	0.805
Acceptable (Unstandardized)	0.605	0.491	0	1
Political Knowledge	2.269	1.406	0	6
Self-Monitoring Propensity	14.008	1.998	9	19
Observations	119			

viewed as being associated with state propaganda. The group mean comparison shows that the treatment significantly lowered respondents' perceptions of congressional responsiveness.

Baseline Result Because the treatment is not a weak one (i.e., it was effective in swaying perceptions of congressional responsiveness), if the social contract hypothesis is correct, it should lead people to express *lower* tax compliance. I find supporting evidence that the treatment significantly moved people's tax compliance downward. Figure 4.1 shows that the informational treatment had a backfire effect on respondents' willingness to comply with tax regulations. The coefficient sign of the treatment is consistently negative for all the outcome measures. The results remain intact after controlling respondents' background characteristics. More importantly, the treatment estimates are statistically significant at the .05 level for two of the three outcome measures. Even though one outcome variable—Acceptable—does not reach the standard significance level, it is very close to the .1 level.

Table 4.2: Balanced Test

	MBA	Non-MBA	Pooled
Female	-0.0183 (0.974)	0.509 (0.395)	0.264 (0.506)
Age	0.0106 (0.899)	0.0286 (0.528)	0.0212 (0.581)
CCP	-0.796 (0.139)	0.0662 (0.911)	-0.369 (0.344)
Income	-0.116 (0.301)	-0.204 (0.184)	-0.150 (0.101)
Political Knowledge	-0.0409 (0.836)	0.128 (0.601)	0.0506 (0.741)
MBA			0.0527 (0.897)
Constant	1.021 (0.720)	0.0121 (0.994)	0.286 (0.818)
<i>N</i>	64	55	119

p-values in parentheses

⁺ $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$

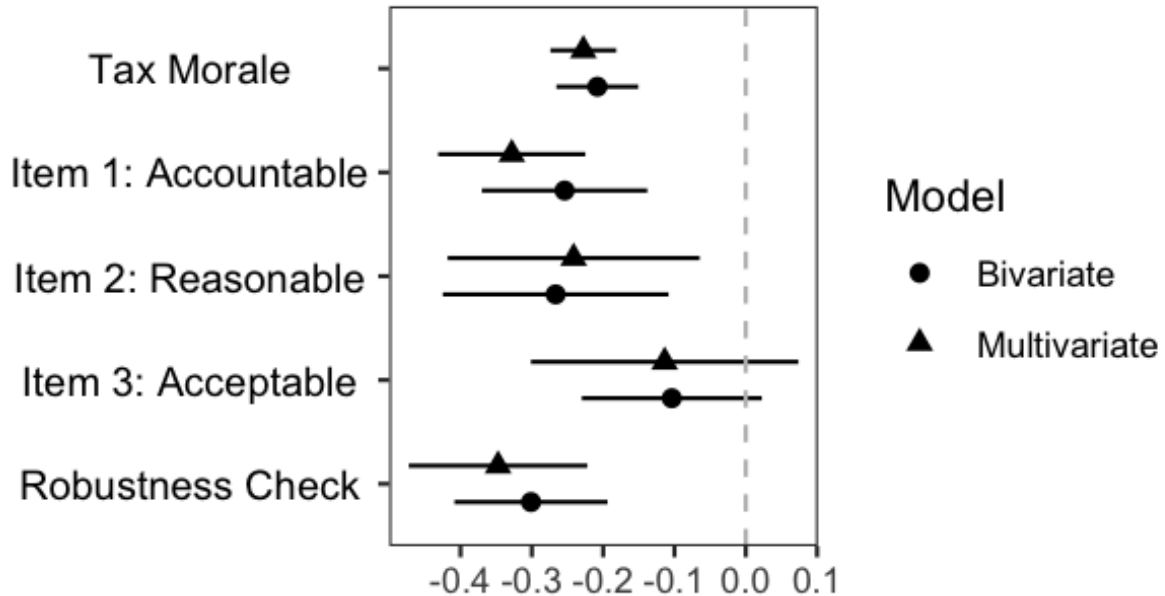
This is likely caused by the small sample size. For robustness check, I conduct a principal component analysis, using the first estimated principal component as the outcome variable. The treatment estimates remain negative and statistically significant at the .05 level.⁵

Overall, the results support my main hypothesis (H1) that individuals' perceptions of government responsiveness will influence their tax compliance. Because many respondents negatively processed and reacted to the informational treatment, those exposed to the information had lower tax compliance than those not exposed. In a nutshell, the political information viewed as hard propaganda exerted a backfire effect.

Effect Heterogeneity Is the magnitude of the backfire effect conditional on one's political knowledge (H2) and/or self-monitoring propensity (H3)? H2 posits that the backfire effect should be stronger among politically sophisticated respondents because they have better

⁵TableC.1 in Supplementary Information reports all the regression estimates.

Figure 4.1: Treatment Effects



Note: The dots represent point estimates and the bars 95% confidence intervals. Tax Morale is an index averaging the standardized scores of the three survey items used to measure compliance attitudes. Robustness check is a principal component analysis using the first principal component as the outcome. Bivariate model regresses respondents' outcomes on the binary treatment condition; multivariate model regresses respondents' outcomes on their treatment condition and background characteristics. I use linear regression model to estimate the treatment effects. $N=119$.

abilities to counterargue dissimilar information. Results reported in Table 4.3 shows no support for this hypothesis. If anything, the negative treatment effects seem mainly driven by those respondents with low political awareness.

Table 4.3: Treatment Effects Moderated by Political Sophistication

	(1)	(2)	(3)	(4)
	Tax Morale	Accountable	Reasonable	Acceptable
Treatment	-0.309* (0.019)	-0.368* (0.035)	-0.319+ (0.070)	-0.240* (0.016)
High Knowledge	-0.161+ (0.079)	-0.435* (0.030)	0.0309 (0.849)	-0.0781 (0.543)
Treatment# High Knowledge	0.240 (0.172)	0.298 (0.186)	0.113 (0.475)	0.309 (0.129)
Constant	0.173** (0.001)	0.308** (0.003)	0.126* (0.028)	0.0856 (0.173)
Obs.	119	119	119	119

p-values in parentheses

+ $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$

Table 4.4: Treatment Effects Moderated by Self-Monitoring Propensity

	(1)	(2)	(3)	(4)
	Tax Morale	Accountable	Reasonable	Acceptable
Treatment	0.0109 (0.935)	-0.482** (0.006)	0.135 (0.445)	0.379+ (0.062)
Low Self-Monitoring	0.302+ (0.076)	0.0150 (0.932)	0.247 (0.190)	0.643** (0.009)
Treatment#Low Self-Monitoring	-0.334 (0.114)	0.418+ (0.056)	-0.680** (0.003)	-0.742* (0.040)
Constant	-0.0926 (0.223)	0.122 (0.191)	-0.0255 (0.856)	-0.375* (0.024)
Obs.	119	119	119	119

p-values in parentheses

+ $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$

H3 posits that the backfire effect should be stronger among low self-monitors because these people are more willing to reveal their truthful answers to sensitive questions. Results

in Table 4.4 show that, first, the estimated coefficients of the interaction variable have the correct direction in three of the four models (Model 1, Model 3, and Model 4) and are statistically significant for two outcome variables—Reasonable and Acceptable. Nevertheless, the estimate of the interaction variable in Model 2 has the wrong direction. In sum, even if some evidence shows that the backfire effect is larger among low self-monitors, one should be cautious in interpreting the results. My study may be inadequately powered because of the small sample size.

Together, my main results support both social contract theory and backfire effects of exposure to dissimilar information. Specifically, I show that even in authoritarian countries people's perceptions of government responsiveness also matter with regard to how much they intend to comply with tax regulations. I also find that persuasive messages can backfire if message recipients question their intent and accuracy. Some evidence further suggests that the backfire effect is particularly salient for low self-monitors.

4.6 Discussion

Why did the informational treatment have a negative impact on people's tax compliance attitudes? One plausible answer is that the information recipients did not believe the information and counterargue against it. I presented open-ended questions in a pilot study to gain understanding of people's opinions about the informational treatment. Although the subjects in the pilot study differ from those in the main experiment, still some similarities hold: All respondents share the same academic training (either business administration or economics), and a majority of respondents are from the same city—Shanghai.

In answering the open-ended questions, most people made negative comments on the received message. Some regarded the messages as government's propaganda effort aiming to convince citizens that China's governing institutions are more responsive. Others suspected the authenticity of the messages, noting that the message likely came from the Chinese government and was intended to induce people to believe congressional respon-

siveness, which in their views is far from the case. Still others mentioned they rarely feel the existence of local congress delegates in their daily life and thus concluded that the message does not reflect the reality. Some people further noted that they do not trust official reports and propaganda materials like the message they just read. They also considered local congressional elections opaque even if the government constantly emphasizes the value of the elections and the benefits citizens can receive after they occur. The qualitative evidence is compatible with Stockmann (2013), who claims that Chinese citizens tend to be resistant to a political message when its source is related to the government (e.g., official newspapers).

Another possible explanation of the negative information processing is that people lack trust in the political actors in the vignette. The existing literature on persuasion has shown that when people perceive the actors in a persuasive message as trustworthy, they are more likely to accept the message (Lupia and McCubbins, 1998). Some studies have shown that Chinese people's levels of political trust hinge on the hierarchy of governments: The central government is often perceived as more trustworthy than local governments (Li, 2004). Statistical results based on the 2008 China Survey also support this argument. Table 4.5 reports that respondents have significantly higher confidence in the central than in local governments.⁶ Tang (2016) also lends empirical evidence that the central government is generally perceived to be highly responsive to popular needs. Therefore, a case can be made that the informational treatment in this study could have successfully persuaded the information recipients had the key political actor in the vignette been the central government.

Table 4.5: Political Trust by Levels of Government

Variable	Mean	Std. Dev.	Min.	Max.	N
Central Government	8.170	2.197	0	10	3763
Local Government (County/City)	6.541	2.653	0	10	3702

⁶The mean scores are calculated by using a survey question asking respondents how much confidence they have in the Central and local governments. This question is coded in an interval scale from 0 to 10; 0 refers to least confident and 10 refers to most confident.

Moreover, the timing of this study may partially contribute to the backfire effect observed. In September 2016, a total of 454 members of the Liaoning province congress left their positions in the aftermath of widespread allegations of vote buying and bribery. Earlier that year, the Chinese government launched an investigation of this case, resulting in the ouster of former Liaoning provincial Communist Party head Wang Min from the party. This salient event may negatively affect how people process the information about responsiveness and representation of local congress delegates. The Liaoning event may lead people to call any persuasive messages concerning improved congressional responsiveness into question. Note that all these possible explanations are suggestive and need to be systematically evaluated in future research.

4.7 Conclusion

Even when individuals' willingness to pay taxes has long been argued in fiscal contract theory to depend on their perceptions of government responsiveness, whether the theory can be applied to authoritarian contexts has been examined in very few studies. I conduct an experimental investigation to test this theory in China and find supporting evidence. A more interesting finding is perhaps that my study participants negatively processed the information about China's improved congressional representation and as a consequence the information had a backfire effect on citizen compliance. My qualitative evidence suggests that Chinese people seem to treat the information as associated with state propaganda. The results are not only consistent with a series of complementary belief-preserving mechanisms (Kunda, 1990),⁷ but are in harmony with new studies showing that hard propaganda tends to backfire.

⁷For example, people's prior beliefs will affect how they process a new information; people tend to exert effort to counterargue against evidence not congruent with their priors, resulting in the adoption of an opposite position instead.

Appendices

Appendix A

Supplementary Information for "Family Matters"

Section A

Public Employment, Proregime Bias, and Parental Transmission. Panel (A) shows that public employees have stronger proregime attitudes than nongovernment employees. All regressions include personal characteristics as controls and are based on 10 multiple-imputed datasets. Panel (B) focuses on the respondents in this study, showing that affiliated subjects are more likely to apply for a CCP membership than nonaffiliated subjects

Table A.1: Intergenerational Transmission

Panel A: Public Employment and Political Attachment

VARIABLES	(1) Trust	(2) Satisfaction	(3) Pride
Public Employee	0.307* (0.178)	0.349** (0.143)	0.092* (0.048)
Local Hukou	0.499*** (0.106)	0.287*** (0.088)	-0.012 (0.027)
Ethnicity: Han	-0.297** (0.150)	-0.098 (0.117)	0.165*** (0.040)
Gender: Male	-0.285*** (0.088)	0.166** (0.073)	0.052** (0.023)
Age	0.022*** (0.003)	0.021*** (0.002)	0.001 (0.001)
Constant	7.256*** (0.211)	6.943*** (0.170)	3.143*** (0.052)
Observations	3,989	3,989	3,989

Panel B: Odds Ratio of Applying for Party Membership

VARIABLES	Party Membership Application
Affiliated Students =1	1.580** (0.281)
Residence (Rural)	0.728** (0.096)
Ethnicity (Han)	1.231 (0.277)
Gender (Male)	0.775** (0.079)
Birth Year	0.948*** (0.017)
CCP Father	1.387** (0.203)
CCP Mother	0.901 (0.140)
Observations	2,054

Robust seeform in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Section B

Government Documents. This section discusses five government documents related to the Eighth Curriculum Reform. I outline each of them below in chronic order.

- *"Decision on Deepening Education Reform, and Promoting Comprehensively the Quality Education"* was issued by China's State Council on June 3, 1999.¹ It marks the beginning of the planning phase of the curriculum reform and, in particular, calls for a reform of the curriculum content.
- *"Decision on Basic Education Reform and Development"* was released by the State Council on May 29, 2001.² It reemphasizes that basic education plays a strategic role in facilitating socialist modernization. In Section 3, the document briefly introduces government objectives for the curriculum reform, such as teaching students (socialist) democracy, the rule of law, and collectivism.
- *"Outlines of Basic Education Reform"* was issued by China's Ministry Education on June 8, 2001.³ This is the first main document in which the government outlines the general objectives for the high school curriculum reform. It also notes that the government would establish a corresponding curriculum framework to support the implementation of the reform.
- *"Suggestions on Strengthening the Ideological and Moral Construction of the Youth"* was issued by the State Council on February 26, 2004.⁴ It describes the political objectives that should be achieved through the high school politics curriculum.
- *"Curriculum Framework for the High School Politics Subject,"* was issued by the Ministry of Education on March 2, 2011.⁵ It presents the specific goals of the new politics curriculum. Notably, it serves as the guiding framework for textbook authors, high school

¹See http://www.moe.gov.cn/jyb_sjzl/moe_177/tnull_2478.html (Retrieved: 05/01/20).

²See http://www.gov.cn/gongbao/content/2001/content_60920.htm (Retrieved: 05/01/20).

³See http://www.moe.gov.cn/srcsite/A26/jcj_kcj_cgh/200106/t20010608_167343.html (Retrieved: 05/01/20).

⁴See http://www.gov.cn/gongbao/content/2004/content_62719.htm (Retrieved: 05/01/20).

⁵See http://www.moe.gov.cn/srcsite/A26/s8001/200403/t20040302_167352.html (Retrieved: 05/01/20).

teachers, exam preparation book publishers, and high school students (Cantoni et al., 2017).

Added Sections (Items) in the New Textbooks (College Entrance Exam). Using data from a text analysis conducted by Cantoni et al. (2017), I list the added sections (items) in the new textbooks (college entrance exam) with respect to the categories of interest below.

- CHINESE POLITICAL INSTITUTIONS

- Added Sections in the New Textbooks

- * Main components of political life
 - * How to participate in political life
 - * The choice of election methods and its basis
 - * Various ways of participating in democratic decision-making
 - * Citizens' direct participation in democratic decision-making
 - * The most comprehensive democratic practices in China
 - * Orderly and disorderly political participation

- Added Items in the Exam Framework

- * Chinese citizens' rights of democratic supervision
 - * Basic principles and content of Chinese citizens participating in political life
 - * Channels for Chinese citizens to participate in political life
 - * China's election system and method
 - * Various ways for citizens to participate in democratic decision-making
 - * The significance of citizens directly participating in democratic decision-making
 - * The meaning and significance of Chinese villages and urban dwellers governing themselves

- CHINESE ECONOMIC INSTITUTIONS

- Added Sections in the New Textbooks

- * Limitations of market allocation of resources
 - * Basic characteristics of the socialist market economy
 - * Strengthening the state's macroeconomic regulations and controls
 - * Functions of fiscal policies
 - * How to correctly utilize fiscal policies
 - * The concept of public goods

- Added Items in the Exam Framework

- * Market adjustment and its limitations
 - * Market allocation of resources
 - * Basic characteristics of the socialist market economy
 - * Public finance and infrastructure construction
 - * Public finance and macroeconomic regulations and controls
 - * Public finance and the guarantee of people's living standards

- GOVERNANCE

- Added Sections in the New Textbooks

- * Various methods of democratic supervision
 - * Responsible exercise of the supervision right
 - * A government that benefits its people
 - * Ways to seek help; legal channels to voice complaints
 - * The specific requirements for government to adhere to the rule of law
 - * The significance of restricting and supervising government's power

- * Cheers for the 'Sunshine Project' (local government operational transparency project)
- * Where does the government's authority come from?
- Added Items in the Exam Framework
 - * Chinese citizens' rights of democratic supervision
 - * The legal channels to conduct democratic supervision
 - * Citizens need to exercise the right of democratic supervision in a responsible manner
 - * The duties of the Chinese government
 - * The fundamental guidelines of the Chinese government; the basic principles of government operations
 - * The significance and requirement of the rule of law
 - * To improve the government's ability to adhere to the rule of law
 - * The significance of restricting and supervising government's power
 - * China's administrative supervision system
 - * The origin and establishment of the Chinese government's authority

Translated Excerpts from the New Textbooks. This section provides translated excerpts from the new textbooks to show how the textbook content related to the outcome of interest was presented under the new politics curriculum. The textbook excerpts are complementary to evidence of changes in the frequency of words across curricula, which does not reveal the context surrounding the relevant words, discussed in the main text.

- CHINESE POLITICAL INSTITUTIONS

- The development of socialist democracy is an important goal for socialist modernization. To achieve this objective, we must be under the leadership of the Party

and popular sovereignty, governing the country by law (excerpted from "Main components of political life").

- An essential guarantee for all decisions to meet the fundamental interests of the public is to elect people who represent the will of the people in democratic elections to join the decision-making institutions and participate in the review, supervision, and formulation of policies. (excerpted from "Various ways of participating in democratic decision-making").
- Citizen participation in the decision-making process through various channels and approaches is crucial to putting decision-making on a more scientific and democratic basis (excerpted from "Citizens' direct participation in democratic decision-making").
- Developing grassroots democracy and ensuring that people enjoy democratic rights are fundamental to the country's development of socialist democracy. ...According to the Organic Law of the Villagers' Committees of the People's Republic of China, voting for the members of the Villagers' Committee directly is an important basis of villagers' self-governance. ...According to the Organic Law of the Urban Residents' Committee of the People's Republic of China, the members of the Urban Residents' Committee are democratically elected by residents (excerpted from "The most comprehensive democratic practices in China").
- Citizens' orderly and disorderly political participation differs in whether they engage in democracy in line with the Constitution, laws, regulations, and procedures; whether they exercise political rights and fulfill political obligations in accordance with the law. ...Following the Constitution, laws, regulations, and procedures ensures orderly participation; otherwise disorder, and even chaos, can be the result (excerpted from "Orderly and disorderly political participation").

- CHINESE ECONOMIC INSTITUTIONS

- Market allocations are far from omnipotent. ...The reliance on market allocations entirely would cause reduced efficiency of resource distributions, giving rise to the waste of resources; the society and economy become unstable, and economic fluctuations as well as chaos occur. There will be unfair income distribution, widening income inequality, and severe polarization (excerpted from "Limitations of market allocation of resources").
- The capitalist market economy is based on private ownership. Upholding the dominant position of public ownership is the foundation of socialist market economy. ...Socialist market economy enables the country to stretch its advantage of aggregating human, physical, and financial resources, making the country's macro-adjustment and intervention better and more effective (excerpted from "Basic characteristics of the socialist market economy").
- Macroeconomic regulations and controls are the government's main responsibilities and functions. ...The country adopts various methods to macro-adjust the national economy. ...The country applies economic, legal, and administrative means to implement the objectives of macroeconomic control (excerpted from "Strengthening the state's macroeconomic regulation and control").

- GOVERNANCE

- When people exercise their power of supervision, they must have the courage to fight against evil and use the supervisory power stipulated in the Constitution and laws for the benefit of the country and the people. Additionally, they must adopt legal approaches and do not interfere with government activities (excerpted from "Responsible exercise of the supervision right").
- The government should exercise its power in accordance with the law and be subject to people's supervision. The country is striving to build a government with

legal authority and responsibilities, strict and impartial law enforcement, openness and justice, integrity and efficiency, and law-abidance and honesty based on the rule of law (excerpted from "A government that benefits its people").

- The government provides various channels for citizens to seek assistance or file complaints, such as setting up hotlines, building up a petition system, and establishing an administrative adjudication system. Citizens should learn to turn to or file complaints to government departments in accordance with the law, which can help solve their own difficulties, safeguard their rights and interests, and keep improving the government in terms of administrative efficiency (excerpted from "Ways to seek help; legal channels to voice complaints").
- The country operates according to law and endeavors to build a socialist country with the rule of law. The authority of the government is granted by law, and the exercise of administrative powers must be based on the Constitution and laws. This is the so-called administration by law, which is a basic requirement for policy implementations of the government and the principle of being responsible to its people (excerpted from "The specific requirements for government to adhere to the rule of law").
- The country has established a comprehensive administrative supervision system on the basis of the Constitution and laws. ...The government under supervision is a guarantee for adhering to the rule of law and administrative efficiency (excerpted from "The significance of restricting and supervising government's power").
- To better serve the people, the government has been promoting transparency in government activities and public affairs. ...Such transparency helps regulate the behavior of the government and its officials and enhance the government's credibility (excerpted from "Cheers for the 'Sunshine Project'").

- In the socialist country led by the CCP, the government, which comes from and serves the people, wins the support of the majority of the people; it represents an incomparable authority that no governments in history could achieve. The authority of a government is marked when its management and services are recognized and accepted by the people (excerpted from "Where does the government's authority come from?").

Section C

Sources of Data. Readers may have noticed that the sample size varies for models using the *Intervention*, *Democracy*, and *Trust* outcomes. This is caused by the fact that the Chinese General Social Survey may change questions across survey waves. Specifically, the *Intervention* questions were asked in all four waves. The *Democracy* question was asked only in the 2013 wave. The *Trust* question has fewer observations because only a subset of respondents in the 2012 wave was asked the question. Table A.2 indicates which questions were asked in each wave with a check mark and the associated number of observations.

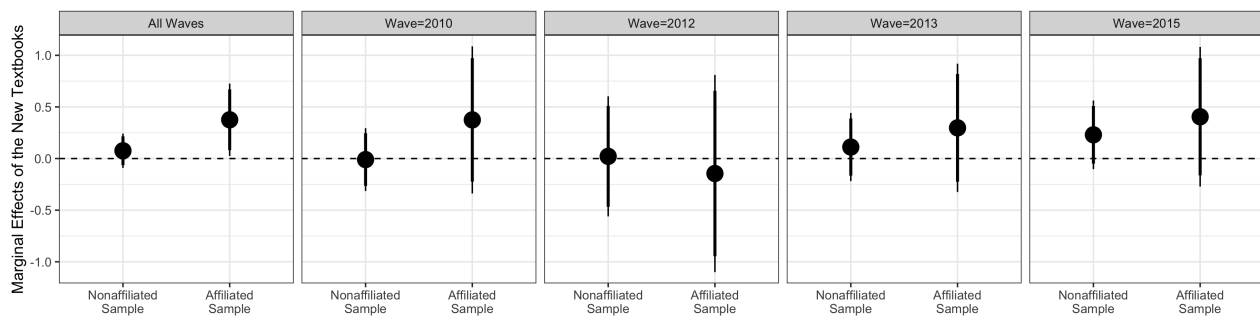
Table A.2: Sources of Observations from Survey Waves

	Intervention	Democracy	Trust
2010 Round	✓ (N=392)	✗	✗
2012 Round	✓ (N=521)	✗	✓ (N=275)
2013 Round	✓ (N=584)	✓ (N=536)	✗
2015 Round	✓ (N=578)	✗	✗
Total N	2075	536	275

The complete absence of data on particular variables in some survey waves means that I cannot multiply impute for the missingness. However, I conduct two analyses regarding *Intervention* to evaluate the robustness of my results. First, I reestimate the effects on *Intervention* when restricting each model to the number of observations available across indicators in that wave. For example, when analyzing the 2012 wave, I restrict the sample to the 275 respondents who responded to *Trust*. For the 2013 wave, I focus on only the 536

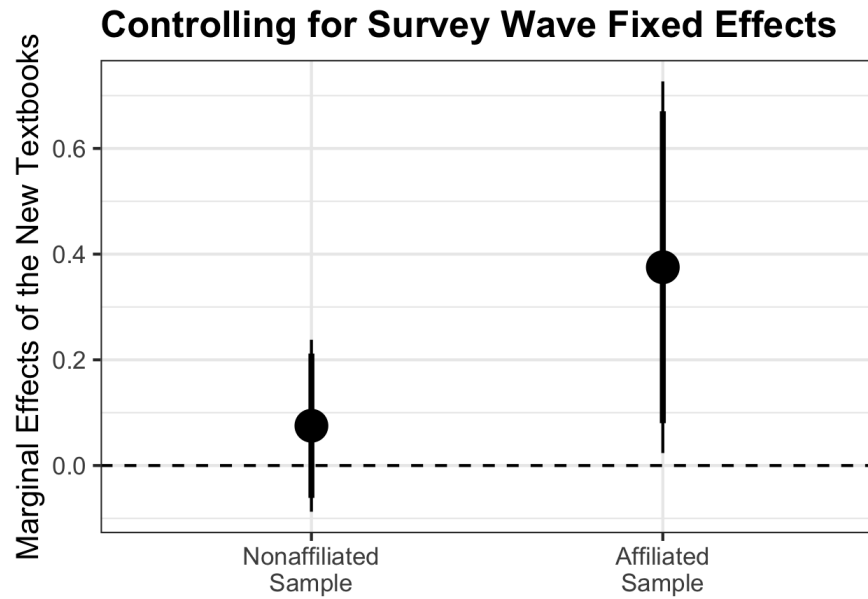
respondents who are available for the analysis in which *Democracy* is the outcome. In the waves where neither *Trust* nor *Democracy* was recorded, I simply use the respondents from the survey wave. Figure A.1 reports the separate regression coefficients with corresponding confidence intervals at the .05 and .1 level. Results show that the estimated marginal effects within each wave are similar to the baseline results, except for the 2012 wave. As expected, the standard errors become larger as a result of fewer observations. I thus prefer a larger sample for the analysis of Intervention (i.e., N=2075) in order to have more precise estimates

Figure A.1: Marginal Effects on *Intervention* by Survey Waves



Second, I reestimate the coefficient, including survey fixed effects to account for time-varying factors that could affect respondents differently across survey waves. Results reported in Figure A.2 are nearly identical to the baseline estimates.

Figure A.2: Marginal Effects on *Intervention* with Survey Fixed Effects



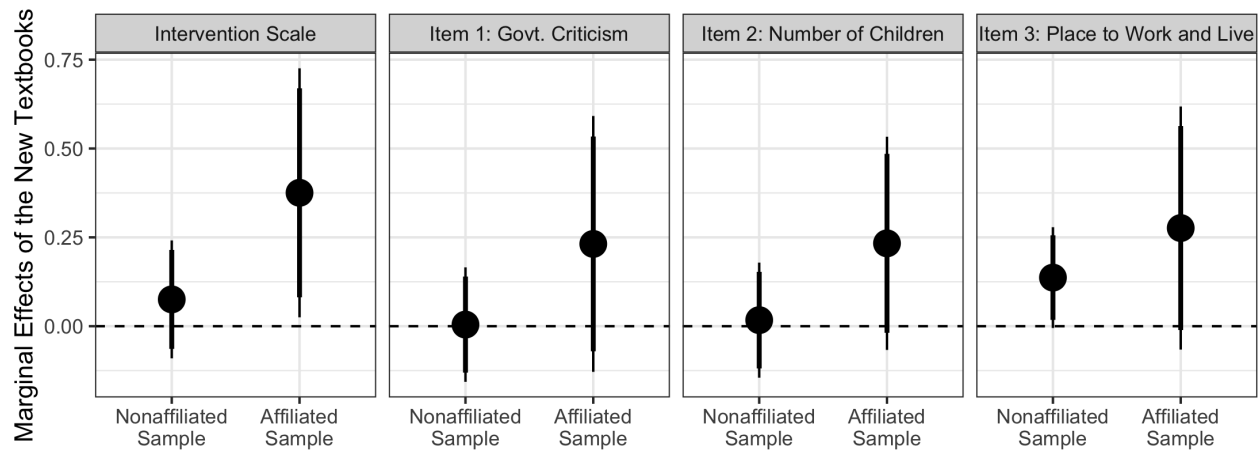
Additional Analyses of Intervention. In the main analysis, *Intervention* is an additive scale based on three survey items related to people’s attitudes toward state intervention in citizen life. Here, I first report the interitem correlations.

	Item 1	Item 2	Item 3
Item 1 (Criticism of Government)			
Item 2 (Number of Children)	0.292		
Item 3 (Place to Work and Live)	0.218	0.190	

Next, I report the baseline results on each item separately. For comparison and consistency, I standardize each item to have a mean of zero and a standard deviation of one. For reference, the leftmost panel in Figure A.3 reports the estimates using the additive scale. Results from the rest of panels show that the estimates for the individual items all have the right direction and follow a consistent pattern —the coefficients are larger among the affiliated sample than the nonaffiliated sample. The three estimates also have similar sizes. None are not statistically significant, however, using a 0.05 two-tailed criterion. One possible explanation

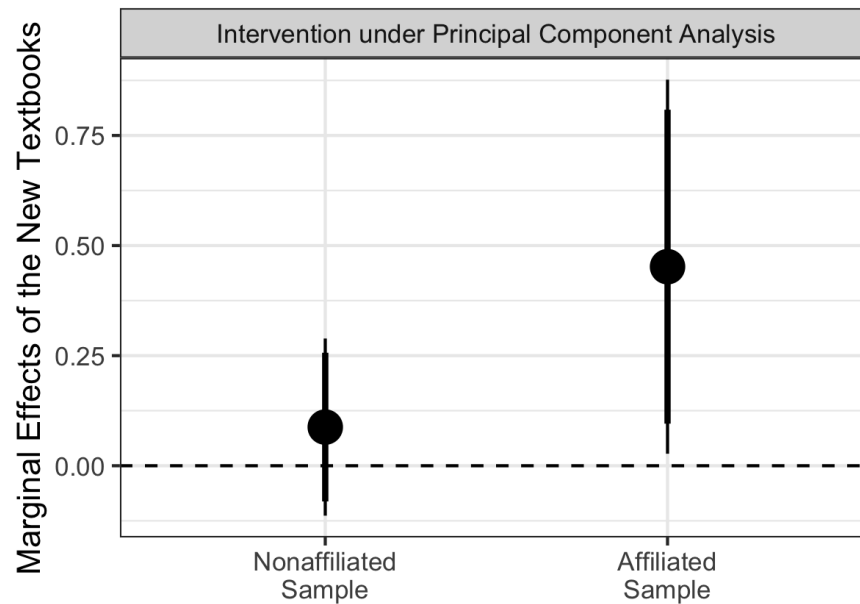
is that the variation of the items is all smaller than that of the additive scale.

Figure A.3: Separate Marginal Treatment Effects on Intervention Items



I also reestimate the baseline model by using principal component analysis. I first estimate the principal components with the three items and find that only the first estimated component, which explains 49% of the total variance of the items, has an eigenvalue greater than one. I then use the first component as the outcome variable to reestimate the treatment effect. Figure A.4 shows that the new estimates are almost identical to the baseline estimates using the additive scale.

Figure A.4: Marginal Treatment Effects on *Intervention* Using PCA



Section D

Balance Test. Table A.3 reports the results of difference-in-means. Accounting for province and cohort fixed effects, the OLS estimates show that the treatment and control groups are indistinguishable in several personal characteristics.

Main Results. Table A.4 shows the OLS estimates of the effects of the new textbooks on attitudes.

Marginal Effect by Affiliation Status. Table A.5 reports the OLS estimates of the marginal effects of the new textbooks, calculated based on the results from the baseline model, on each of the two groups (affiliated and nonaffiliated samples).

Falsification Tests. Table A.6 reports the OLS estimates using placebo attitudes (Panel A)

and placebo reform years (Placebo B)

Table A.3: Balance Test

VARIABLES	Exposure to the New Curriculum
Gender: Male	0.006 (0.009)
Ethnicity: Han	0.007 (0.007)
Height	-0.002 (0.009)
Rural Residence	-0.003 (0.006)
Father is CCP Member	0.001 (0.008)
Mother is CCP Member	0.008 (0.008)
Father's Educational Level	0.004 (0.007)
Mother's Educational Level	0.009 (0.007)
Affiliated Father	0.003 (0.006)
Affiliated Mother	-0.006 (0.006)
Constant	0.018 (0.039)
Province FE	✓
Cohort FE	✓
Observations	2,026
R-squared	0.756

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table A.4: Main Results

VARIABLES	(1) Government Intervention	(2) Socialist Democracy	(3) Trust in Officials	(4) Government Intervention	(5) Socialist Democracy	(6) Trust in Officials
New Curriculum	0.099 (0.083)	-0.178 (0.155)	0.042 (0.220)	0.075 (0.085)	-0.248 (0.161)	-0.084 (0.230)
Affiliated Students				-0.310*** (0.118)	-0.318* (0.188)	-0.192 (0.257)
New Curriculum × Affiliated Students				0.300* (0.172)	0.830*** (0.207)	0.755** (0.294)
Constant	0.106 (0.068)	-0.066*** (0.014)	0.361 (0.383)	0.124* (0.074)	0.074 (0.069)	0.420 (0.384)
Province FE	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓
Observations	2,075	536	275	2,075	536	275
R-squared	0.043	0.056	0.177	0.047	0.069	0.196

Clustered standard errors in parentheses

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Note: All regression coefficients are standardized. Standard errors are clustered at the province × cohort level.

Table A.5: Marginal Treatment Effects by Affiliation Status

Marginal Effects Comparison	(1) Government Intervention	(2) Socialist Democracy	(3) Trust in Officials
Affiliated Students	0.375** (0.179)	0.582*** (0.215)	0.671* (0.356)
Nonaffiliated Students	0.075 (0.085)	-0.248 (0.161)	-0.084 (0.230)
Province FE	✓	✓	✓
Cohort FE	✓	✓	✓
Observations	2,075	536	275

Clustered Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table A.6: Falsification Tests

Panel A: Placebo Attitudes

VARIABLES	(1) Trust in People	(2) Trust in Relatives	(3) Trust in Neighbors	(4) Trust in Bank Staff	(5) Trust in Journalists
New Curriculum	0.230 (0.190)	0.117 (0.132)	0.039 (0.136)	0.004 (0.141)	0.007 (0.179)
Affiliated Student	-0.048 (0.174)	0.326** (0.125)	0.128 (0.154)	0.312** (0.121)	-0.135 (0.144)
New Curriculum × Affiliated Student	0.209 (0.275)	-0.242 (0.167)	-0.238 (0.207)	-0.313 (0.214)	0.232 (0.255)
Constant	3.363*** (0.129)	3.384*** (0.319)	2.655*** (0.111)	2.889*** (0.114)	2.273*** (0.185)
Province FE	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓
Observations	521	280	280	280	276
R-squared	0.095	0.189	0.142	0.181	0.125

Clustered standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Panel B: Placebo Reform

VARIABLES	(1) Intervention	(2) Democracy	(3) Trust
New Curriculum	0.008 (0.078)	0.067 (0.164)	0.015 (0.269)
Affiliated Student	-0.122 (0.091)	-0.202 (0.181)	-0.007 (0.217)
New Curriculum × Affiliated Student	0.082 (0.131)	0.281 (0.248)	-0.035 (0.378)
Constant	0.046 (0.065)	0.326*** (0.123)	0.796*** (0.255)
Province FE	✓	✓	✓
Cohort FE	✓	✓	✓
Observations	1,796	440	232
R-squared	0.046	0.129	0.189

Clustered standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: The regression coefficients are standardized. Standard errors are clustered at the province × cohort level.

Section E

I conduct a series of additional tests to evaluate the robustness of my results. I discuss each of them below.

- First, one may be concerned about the assumption of my analysis that students start high school at age 15 because some people might start high school earlier or later. I use two strategies to address this crossover concern. Had my results been sensitive to the school entry year assumption, I should not have found any effect in these two analyses.
 - I explore three different bandwidths around the threshold: (1) the cohorts just one year above and below the reform year, (2) a two-cohort bandwidth on each side of the curriculum, and (3) a three-cohort bandwidth on each side of the curriculum. Table A.7 and Figure A.5 show that my results are robust to various bandwidth selections.
 - I reanalyze the data taking people’s birth months into account. In the main analysis I assume people are 15 when they start high school, but sometimes people need to have hit the required age by August 31 to start school; one would wait a whole extra year if her birthday is after that. For example, some aged 16 born September-December had to wait a year and so they actually went through the new textbooks, but in the analysis I code them as pre-reform. I recode some respondents’ treatment conditions in light to their birth years and months, finding that my results are mostly the same (the only exception is *Intervention*; see Table A.8 and Figure A.6).
- Second, even if students cannot self-select parents who work for the government, one may still wonder whether some other feature of these students other than parental occupation explains their differential responses to the treatment. I address this concern by matching parents’ background characteristics that would affect their public

employment status, including CCP membership and educational attainment.⁶ When implementing the entropy balancing method (Hainmueller, 2012), Table A.9 and Figure A.7 show that my results from a reweighted sample are still robust.

- Third, one may wonder whether the inclusion of individual-level controls would overturn the results. I reanalyze the data by adding several individual controls. To avoid posttreatment bias, I first include factors occurring before students start high school (i.e., before exposure to politics textbooks), such as gender, ethnicity, household registration, parents' Party membership, and parents' educational attainment. In an alternative specification, I add a series of posttreatment variables potentially correlated with people's political attitudes, such as a person's educational attainment, frequency of media use, CCP membership, and perceived socioeconomic condition. Table A.10 and Figure A.8 show that the results are stable.
- Fourth, because the introduction dates of the new curriculum were not randomly assigned across provinces, a concern is that factors determining the introduction dates instead of the new curriculum may affect individuals' attitudes. I reanalyze the data by taking into account province-level income in 2003, a powerful predictor of earlier adoption of the new curriculum (Cantoni et al., 2017). Specifically, I control for the interaction between a province's 2003 gross regional product per capita and the cohort fixed effects. Table A.11 and Figure A.9 show that including these controls does not considerably affect my findings.
- Fifth, I reanalyze the data by including a full set of province fixed effects interacted with cohort-level trends. This model specification allows each province to have its own linear trend in attitudes across cohorts, addressing the concern that trends in the attitudes across the cohort in provinces may generate the differences in attitudes

⁶Granted, many other factors could affect public employment status, but the parental information in the survey I can use is rather limited.

that I attribute to the new politics textbooks. Table A.12 and Figure A.10 show that controlling for province-specific, cross-cohort trends does not substantially change the results.

- Sixth, I use a more demanding model, in which the interaction terms between province and cohort fixed effects are included, to address the unobservable province and cohort covarying characteristics. Results reported in Table A.13 show that this model does not qualitatively change my results, although the estimate on *Intervention* becomes less precise because of a great number of new parameters added to the model.
- Seventh, I reanalyze data Using imputed data for missing values. Table A.14 and Figure A.11 show that the results are very similar to the ones using listwise deletion in the main analysis.

Table A.7: Robustness Check: Different Bandwidths

VARIABLES	Intervention			Democracy			Trust		
	One-Cohort (1)	Two-Cohort (2)	Three-Cohort (3)	One-Cohort (4)	Two-Cohort (5)	Three-Cohort (6)	One-Cohort (7)	Two-Cohort (8)	Three-Cohort (9)
New Curriculum	0.287* (0.158)	0.216 (0.135)	0.167* (0.099)	0.227 (0.237)	0.217 (0.232)	-0.162 (0.173)	-0.123 (0.350)	-0.262 (0.322)	-0.039 (0.276)
Affiliated Student	-0.447* (0.240)	-0.318* (0.169)	-0.285** (0.128)	-0.698** (0.330)	-0.173 (0.215)	-0.086 (0.219)	-1.316*** (0.417)	-0.358 (0.428)	-0.270 (0.309)
New Curriculum × Affiliated Student	0.486* (0.280)	0.387* (0.223)	0.287 (0.184)	1.167*** (0.381)	0.733*** (0.256)	0.621** (0.241)	2.048*** (0.495)	0.939* (0.478)	0.990*** (0.352)
Province FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	788	1,108	1,595	209	296	431	103	138	206
R-squared	0.101	0.079	0.058	0.197	0.111	0.078	0.305	0.289	0.233

Clustered standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: This regression table shows the OLS estimates of the effects of the new politics textbooks using three different bandwidths. The regression coefficients are standardized. Standard errors are clustered at the province × cohort level.

Table A.8: Robustness Check: Birth Months

VARIABLES	(1) Intervention	(2) Democracy	(3) Trust
New Curriculum	-0.154*** (0.055)	-0.244 (0.150)	-0.194 (0.210)
Affiliated Student	-0.274*** (0.100)	-0.165 (0.160)	-0.097 (0.216)
New Curriculum × Affiliated Students	0.388* (0.207)	0.726*** (0.211)	0.845*** (0.294)
Province FE	✓	✓	✓
Cohort FE	✓	✓	✓
Observations	2,075	536	275
R-squared	0.049	0.064	0.198

Clustered standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: This regression table shows the OLS estimates of the effects of the new textbooks, taking respondents' birth months into account when coding their treatment conditions. The regression coefficients are standardized. Standard errors are clustered at the province × cohort level.

Table A.9: Robustness Check: Matching Analysis

VARIABLES	(1) Intervention	(2) Democracy	(3) Trust
New Curriculum	0.113 (0.173)	-0.558 (0.343)	0.129 (0.279)
Affiliated Student	-0.282** (0.116)	-0.556*** (0.212)	-0.207 (0.229)
New Curriculum × Affiliated Student	0.376** (0.176)	1.219*** (0.288)	0.642** (0.280)
Province FE	✓	✓	✓
Cohort FE	✓	✓	✓
Observations	2,014	524	271
R-squared	0.128	0.225	0.344

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: This regression table shows the main results using a reweighted sample that matches on parents' public employment status. The regression coefficients are standardized. Standard errors are in parentheses.

Table A.10: Robustness Check: Inclusion of Individual Controls

VARIABLES	Pre-Treatment			Pre- and Post-Treatment		
	Intervention	Democracy	Trust	Intervention	Democracy	Trust
New Curriculum	0.113 (0.088)	-0.217 (0.161)	-0.113 (0.235)	0.109 (0.087)	-0.260 (0.161)	-0.076 (0.240)
Affiliated Student	-0.190 (0.123)	-0.322 (0.195)	-0.183 (0.281)	-0.212* (0.123)	-0.367* (0.194)	-0.127 (0.287)
New Curriculum × Affiliated Student	0.299* (0.174)	0.891*** (0.211)	0.740** (0.343)	0.317* (0.174)	0.875*** (0.213)	0.690* (0.356)
Gender (Male=1)	-0.012 (0.078)	0.294** (0.121)	0.164 (0.246)	-0.010 (0.077)	0.309** (0.131)	0.102 (0.255)
Ethnicity (Han=1)	-0.032 (0.115)	-0.053 (0.182)	0.086 (0.253)	-0.005 (0.111)	-0.084 (0.183)	0.108 (0.263)
Height	-0.003 (0.005)	-0.017* (0.008)	-0.016 (0.012)	-0.002 (0.005)	-0.017* (0.009)	-0.014 (0.012)
Hukou (Rural=1)	0.017 (0.071)	-0.146* (0.087)	0.187 (0.168)	-0.022 (0.072)	-0.158* (0.088)	0.154 (0.168)
CCP Father	0.123* (0.066)	0.057 (0.127)	-0.127 (0.185)	0.165** (0.067)	0.069 (0.130)	-0.113 (0.216)
CCP Mother	-0.266*** (0.078)	-0.046 (0.353)	0.273 (0.253)	-0.199** (0.080)	0.102 (0.339)	0.270 (0.260)
Father's Education	-0.021* (0.012)	-0.038 (0.030)	0.029 (0.030)	-0.020 (0.012)	-0.044 (0.030)	0.037 (0.032)
Mother's Education	-0.029** (0.014)	0.032 (0.028)	0.000 (0.035)	-0.026* (0.014)	0.026 (0.028)	0.009 (0.034)
Edu. Attainment				0.001 (0.010)	0.041* (0.021)	-0.033 (0.029)
Internet Usage				-0.131*** (0.027)	-0.027 (0.050)	-0.093 (0.076)
Social Status				0.022 (0.016)	0.014 (0.040)	-0.029 (0.046)
CCP Membership				-0.171*** (0.060)	-0.093 (0.174)	-0.014 (0.311)
Province FE	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓
Observations	2,010	523	271	1,995	518	270
R-squared	0.068	0.097	0.211	0.087	0.108	0.223

Clustered standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: This regression table reports the OLS estimates of the effect of the new textbooks with pretreatment and posttreatment individual-level controls. Standard errors are clustered at the province × cohort level.

Table A.11: Robustness Check: Addressing Non-Randomized Reform

VARIABLES	(1) Intervention	(2) Democracy	(3) Trust
New Curriculum	0.058 (0.088)	-0.240 (0.172)	-0.020 (0.189)
Affiliated Student	-0.316*** (0.119)	-0.310* (0.187)	-0.088 (0.225)
New Curriculum × Affiliated Student	0.292* (0.172)	0.768*** (0.210)	0.535** (0.256)
Province FE	✓	✓	✓
Cohort FE	✓	✓	✓
Observations	2,075	536	275
R-squared	0.050	0.085	0.227

Clustered standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: This regression table shows the OLS estimates of the effect of the new textbooks accounting for interaction terms between provincial gross regional product per capita in 2003 and the cohort fixed effects. The regressions coefficients are standardized. Standard errors are clustered at the province × cohort level.

Table A.12: Robustness Check: Province-Specific, Cross Cohort Linear Trend

VARIABLES	(1) Intervention	(2) Democracy	(3) Trust
New Curriculum	0.070 (0.081)	-0.267 (0.163)	-0.035 (0.261)
Affiliated Student	-0.305** (0.120)	-0.273 (0.186)	-0.177 (0.291)
New Curriculum × Affiliated Student	0.301* (0.172)	0.786*** (0.211)	0.701** (0.348)
Province FE	✓	✓	✓
Cohort FE	✓	✓	✓
Linear Trend	✓	✓	✓
Observations	2,075	536	275
R-squared	0.062	0.124	0.242

Clustered standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: This regression table reports the OLS estimates of the effects of the new textbooks, accounting for (linear) trend across cohorts within a province. The regression coefficients are standardized. Standard errors are clustered at the province × cohort level.

Table A.13: Robustness Check: A Tighter Model Specification

VARIABLES	(1) Intervention	(2) Democracy	(3) Trust
New Curriculum	-0.691 (0.639)	-0.927 (0.725)	-0.632 (0.595)
Affiliated Student	-0.317*** (0.117)	-0.408* (0.245)	-0.171 (0.407)
New Curriculum × Affiliated Student	0.205 (0.169)	0.943*** (0.307)	1.001** (0.484)
Province FE	✓	✓	✓
Cohort FE	✓	✓	✓
Province FE × Cohort FE	✓	✓	✓
Observations	2,075	536	275
R-squared	0.123	0.284	0.456

Robust standard errors in parentheses

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Note: This regression table presents the OLS estimates of the effects of new textbooks using a tighter model specification, in which a full set of interaction terms between province fixed effects and cohort fixed effects are included. The regression coefficients are standardized.

Table A.14: Robustness Check: Multiple Imputation

VARIABLES	(1) Intervention	(2) Democracy	(3) Trust
New Curriculum	0.079 (0.085)	-0.234 (0.195)	-0.013 (0.233)
Affiliated Student	-0.303** (0.118)	-0.352 (0.219)	-0.185 (0.256)
New Curriculum × Affiliated Student	0.288* (0.170)	0.922*** (0.282)	0.724** (0.295)
Province FE	✓	✓	✓
Cohort FE	✓	✓	✓
Observations	2,092	588	280

Clustered standard errors in parentheses

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Note: This regression table reports the OLS estimates of the effects of the new textbooks based on ten multiple-imputed datasets. The missing values are imputed by *Amelia* in R. The regression coefficients are standardized. Standard errors are clustered at the province × cohort level.

Table A.15: Mechanism Test: Parental Indoctrination

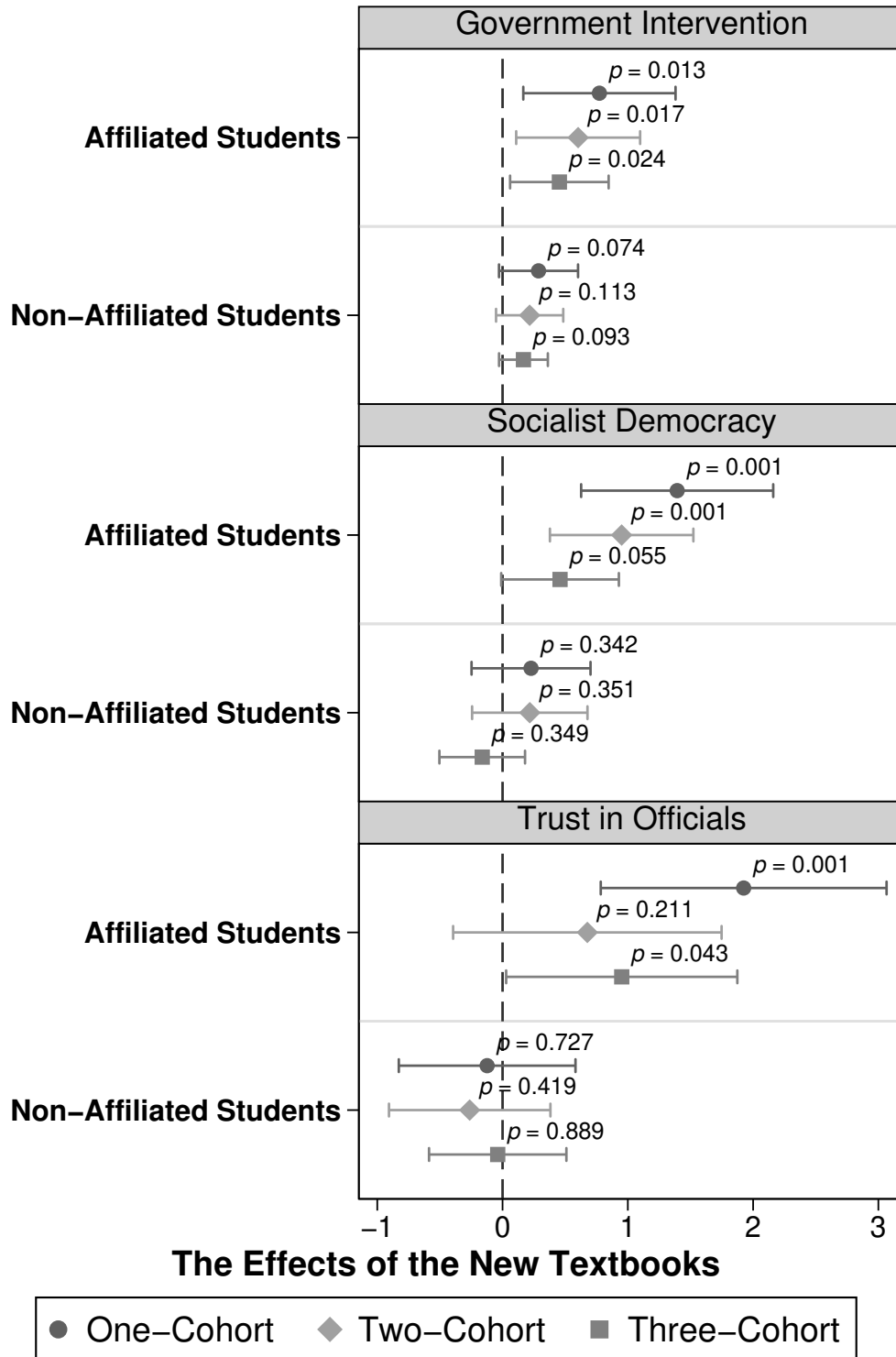
VARIABLES	(1) Intervention	(2) Democracy	(3) Trust
Affiliated Respondents (0 versus 1)	0.014 (0.289)	-0.292 (0.471)	-0.034 (0.607)
Constant	-0.108 (0.089)	-0.088 (0.137)	-0.172 (0.610)
Province FE	✓	✓	✓
Cohort FE	✓	✓	✓
Observations	1,106	268	114
R-squared	0.096	0.157	0.278

Robust standard errors in parentheses

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

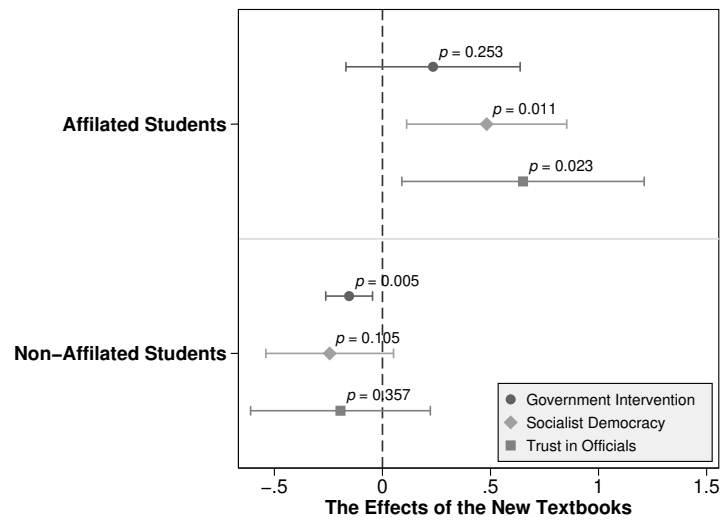
Note: This regression table shows the mean differences in the targeted attitudes between affiliated and non-affiliated respondents who did not attend high school during the reform period. Results are based on a five-cohort bandwidth on each side of the curriculum. The regression coefficients are standardized. Standard errors are clustered at the province \times cohort level.

Figure A.5: Marginal Treatment Effects Using Different Bandwidths



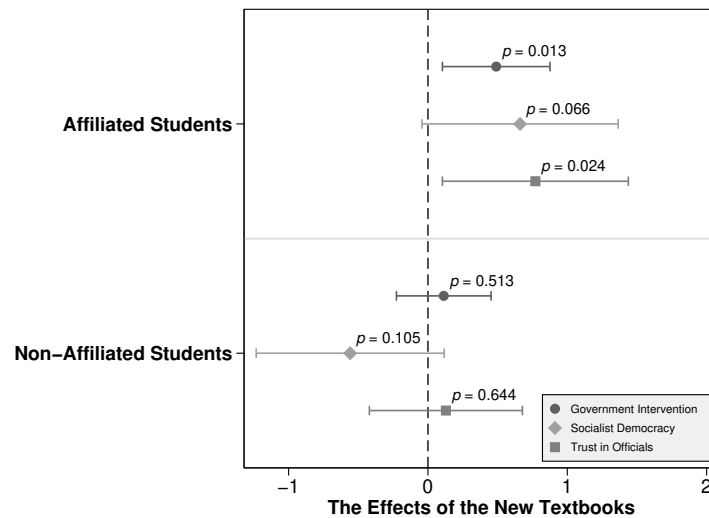
Note: This coefficient plot shows the marginal effects of the new textbooks using different bandwidths (+/-1, +/-2, and +/-3). The bullet symbols represent the standardized coefficients and the bars 95% confidence intervals.

Figure A.6: Marginal Treatment Effects Taking Birth Month into Account



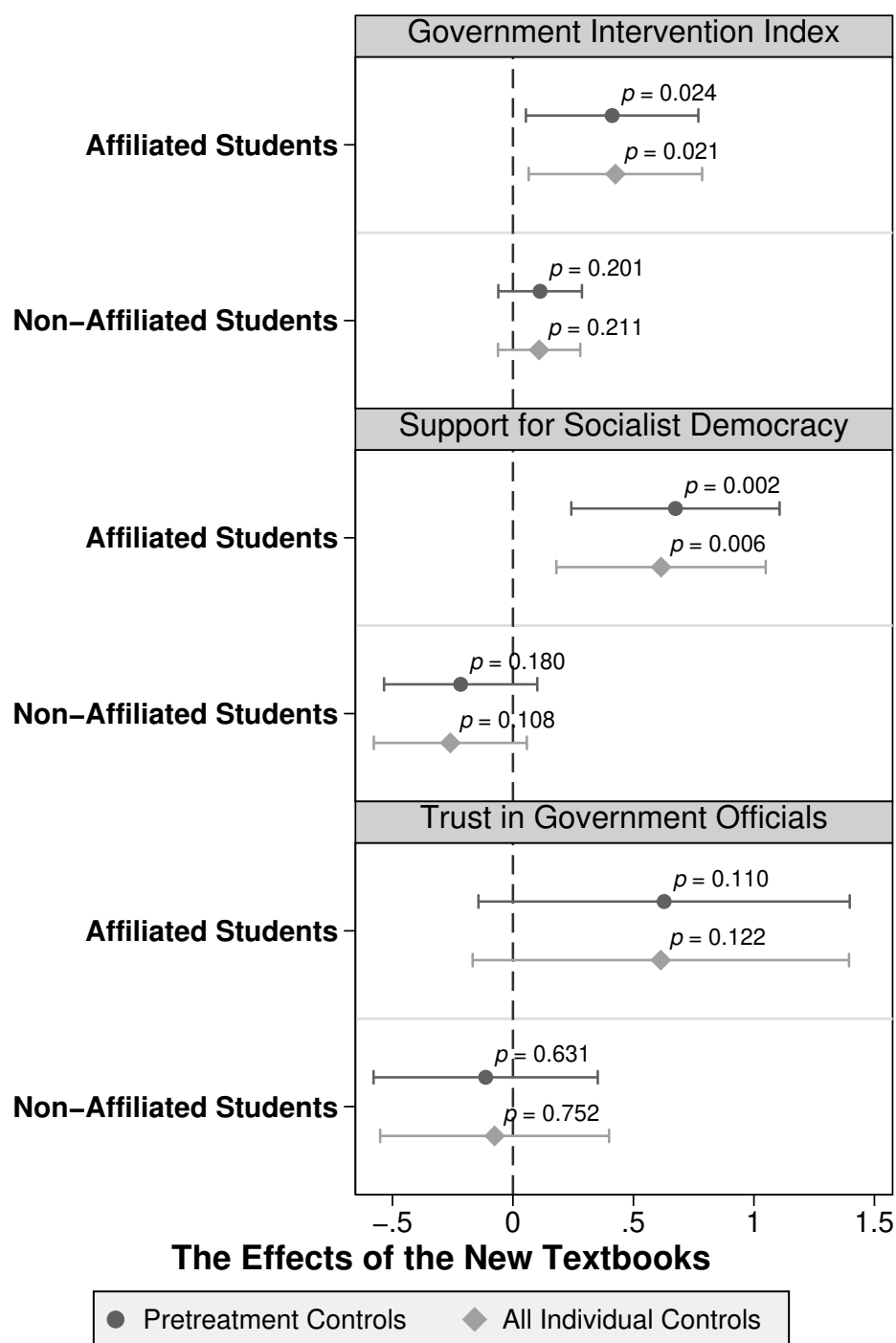
Note: This coefficient plot presents the OLS estimates of the marginal effects of the new textbooks, taking respondents' birth months into account when coding their treatment conditions. The bullet symbols represent the standardized coefficients and the bars 95% confidence intervals.

Figure A.7: Marginal Treatment Effects using Matching Analysis



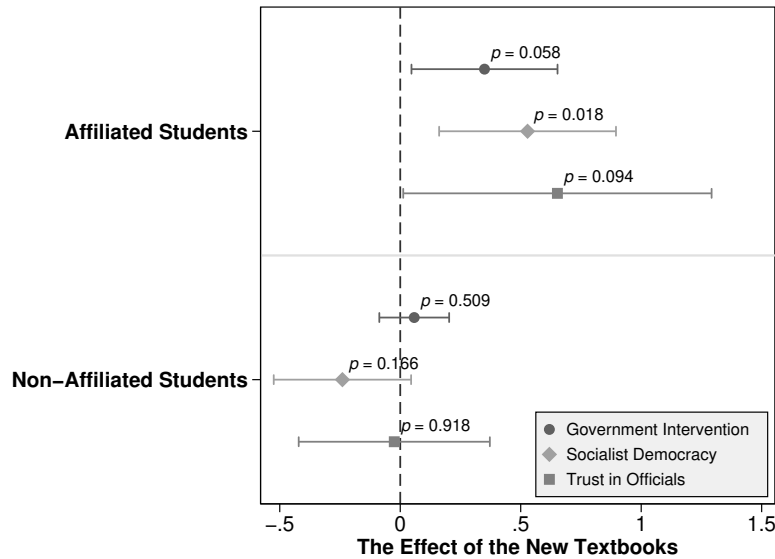
Note: This coefficient plot shows the OLS estimates of the marginal effects of the new textbooks using a reweighted sample generated by the entropy balancing method. The bullet symbols represent the standardized coefficients and the bars 95% confidence intervals.

Figure A.8: Marginal Treatment Effects: Inclusion of Individual Controls



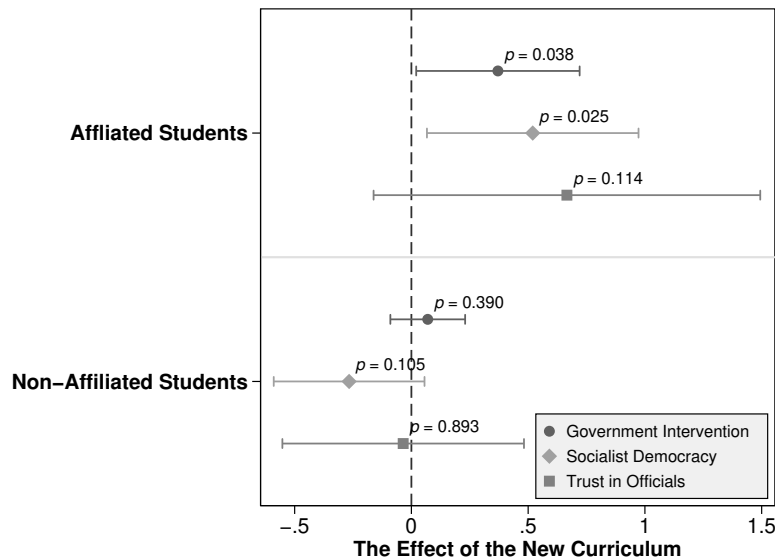
Note: This coefficient plot presents the OLS estimates of the marginal effects of the new textbooks, controlling for a battery of personal characteristics. The bullet symbols represent the standardized coefficients and the bars 95% confidence intervals.

Figure A.9: Marginal Treatment Effects After Addressing Non-Randomized Reform



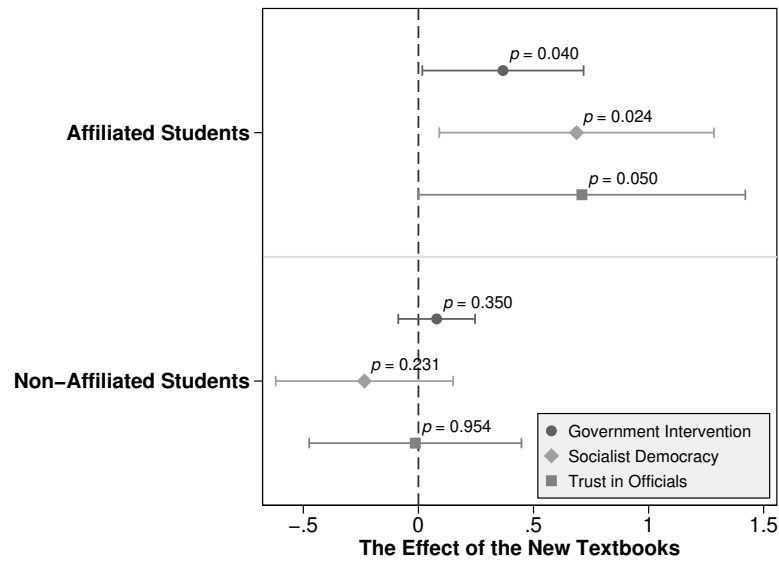
Note: This coefficient plot shows the OLS estimates of the marginal effects of the new textbooks, controlling for interaction terms between provincial GRP Per Capital and cohort fixed effects. The bullet symbols represent the standardized coefficients and the bars 90% confidence intervals.

Figure A.10: Marginal Treatment Effects with Province-Specific, Cross-Cohort Trends



Note: This coefficient plot presents the OLS estimates of the marginal effects of the new textbooks, allowing each province to have its own linear trend in attitudes across cohorts. The bullet symbols represent the standardized coefficients and the bars 95% confidence intervals.

Figure A.11: Marginal Treatment Effects using Imputed Data for Missing Values



Note: This coefficient plot shows the OLS estimates of the marginal effects of the new textbooks. The bullet symbols represent the standardized coefficients and the bars 95% confidence intervals. The regression estimates are based on 10 multiple-imputed datasets to cope with missing values.

Appendix B

Supplementary Information for "How the Pro-Beijing Media Influences Voters"

Figure B.1: Screenshot of the Invitation Email (English-translated)



Dear {xxxxxx}:

Hello, we are a group of researchers from the University of Texas, U.S.A. We would like to let you know that you could receive an extra NT\$150 bonus by browsing our news website. We encourage you to visit the website to get informed about the important news!

[Enter the Website](#)

Thanks again for participating in our research

Sincerely,
Toluna and University of Texas

Figure B.2: Screenshot of the Treatment Website's Front Page (English-translated)

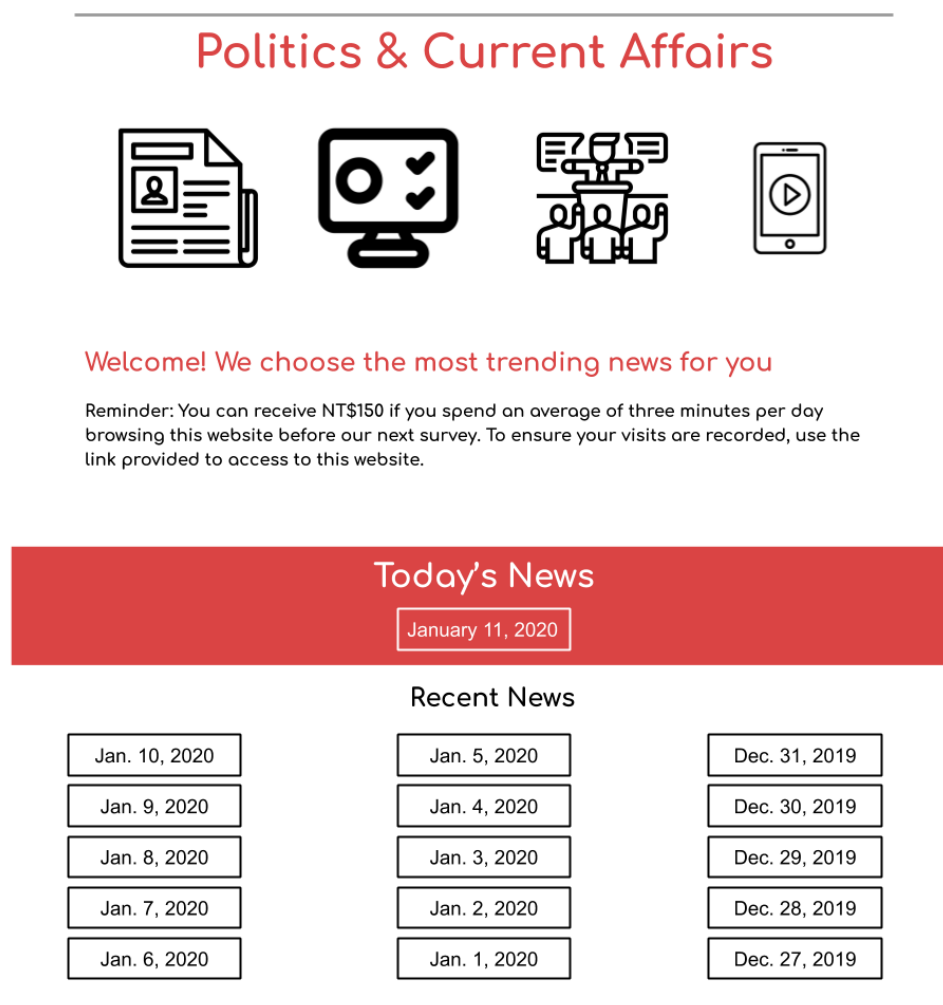
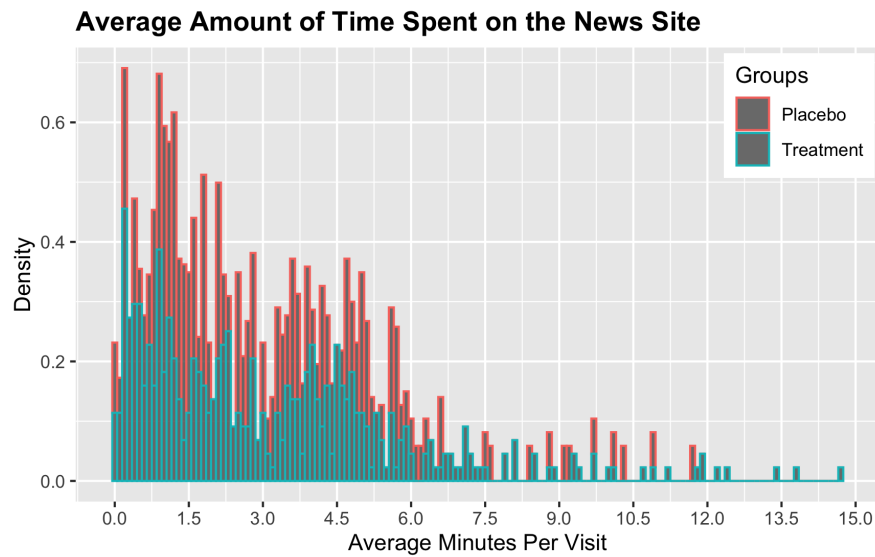
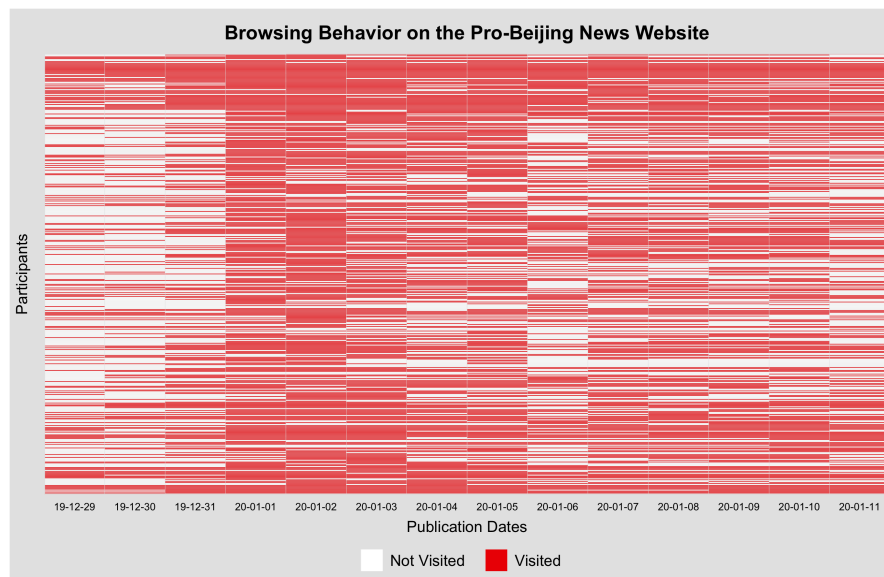


Figure B.3: Distribution of Average Browsing Time



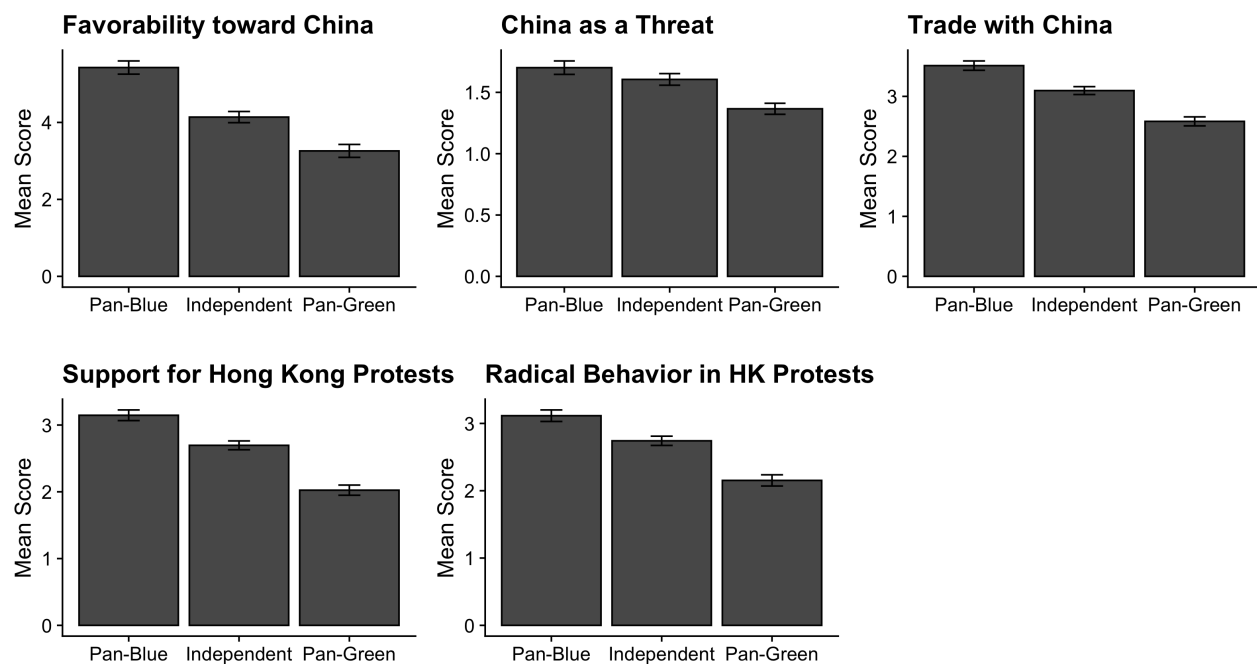
Note: The figure excludes those participants who never return to the site after the first visit and those inactive participants who spend more than 15 minutes per day on average (ten participants, three of them did not participate in the endline survey). In total, the figure contains 609 participants (439 in the treatment group and 170 in the placebo group). For the treatment group the average time is 187.25 seconds; for the placebo group it is 190.51 seconds. I cannot reject the null hypothesis that the average duration of the two groups is indistinguishable (Coefficient= 3.26 seconds (s.e. = 14.08); p-value= .817, two-tailed).

Figure B.4: Panel View of Browsing Behavior across Individual and Time



Note: The figure is a panel view of website browsing activities across individual and time. A red rectangle means that a participant was “treated” by the news articles from a given publication date. The figure excludes participants who never visit the website throughout the study period.

Figure B.5: Baseline Mean Scores of Beliefs on China by Political Priors



Note: The bar represents 95% confidence interval of the mean. I record the survey responses, if necessary, to make higher value represent more positive, favorable to China.

Section A. Headlines of News Articles

Date	Headline
Jan 11	最後一夜韓蔡北高對軌！韓強調最重視南部英老調再賣芒果干 中選會拚今晚10點完成開票！投下神聖一票記得帶三寶 在地產業投資力加大人才需求高上海大學畢業生起薪3.1萬元逼近台 示威不停1／5港人患潛在憂鬱症 台商返鄉投票加班機創10年新低 兩岸和平或衝突就看今朝！民進黨喊保台或賣台都是假議題
Jan 10	決戰24小時禿漢燕合體藍大團結百萬庶民挺韓 送韓國瑜進總統府全民公憤凝聚韓風吶喊下架民進黨 籲知識藍、經濟藍歸隊趙少康喊話投宋就是投蔡 起風了有規定一定要支持到死嗎臉書粉專蔡英文後援會倒戈挺韓 美年報批無視人權陸斥粗暴干預內政 新官上任頻拜會駱惠寧見林鄭重提止暴制亂 聚焦採購智財權金融自由化外匯陸證實劉鶴1月13日赴美簽貿易協定
Jan 9	禿子燕子漢子合體送國政配進總統府庶民站出來今凱道見 選前之夜加碼台中場掃全台蔡稱為台挺腰桿 如果當選還是會吃路邊攤平民風韓喊當庶民總統 只要扯上對岸就可能成滲透來源反滲透法逼退陸資斷台青創業路 百萬在陸台人淪台版羅興亞人 反滲透法將台灣人才推往大陸 預言選後藍綠黨內都會大洗牌傳最後挺韓柯指不表態就是表態 網友惡意抹紅民衆黨告違選罷法
Jan 8	首投族118萬挺韓vs.挺蔡站出來！藍綠強催青年票 選前恐再出大招沒有他們做不到的！防奧步韓籲別骯髒贏選舉 新聞透視》強力「防奧步」！以昔鑑今藍非無的放矢 農會憂陸盤商都是政協怎迴避 政院對面掛抹紅布條韓辦提告 長期批判能源政策部落客陳立誠收警告通知！Google證實政府網軍竊取密碼 挺一國兩制吉國元首開先例 台不能再給蔡4年美媒刊文藍瘋傳 外交圍堵一中論述頻加碼
Jan 7	藍控拿公帑吸收學生當打手！點名謝長廷培訓假韓粉 批故宮政策屬言論自由範疇！法官打臉查水表裁定蘇宏達不罰 劉鶴1月13日赴美簽首階段貿易協定 中聯辦新主任駱惠寧提三個確保 陸外貿利多RCEP將擴大投資 轉貼被查辦婦泣訴天天失眠
Jan 6	黃金周回防新北韓痛批綠貪腐蔡嗆藍阻改革 蔡籲再4年綠委紅不讓 韓嗆下架民進黨藍委全壘打 選前陸對台更多陽光不吹北風

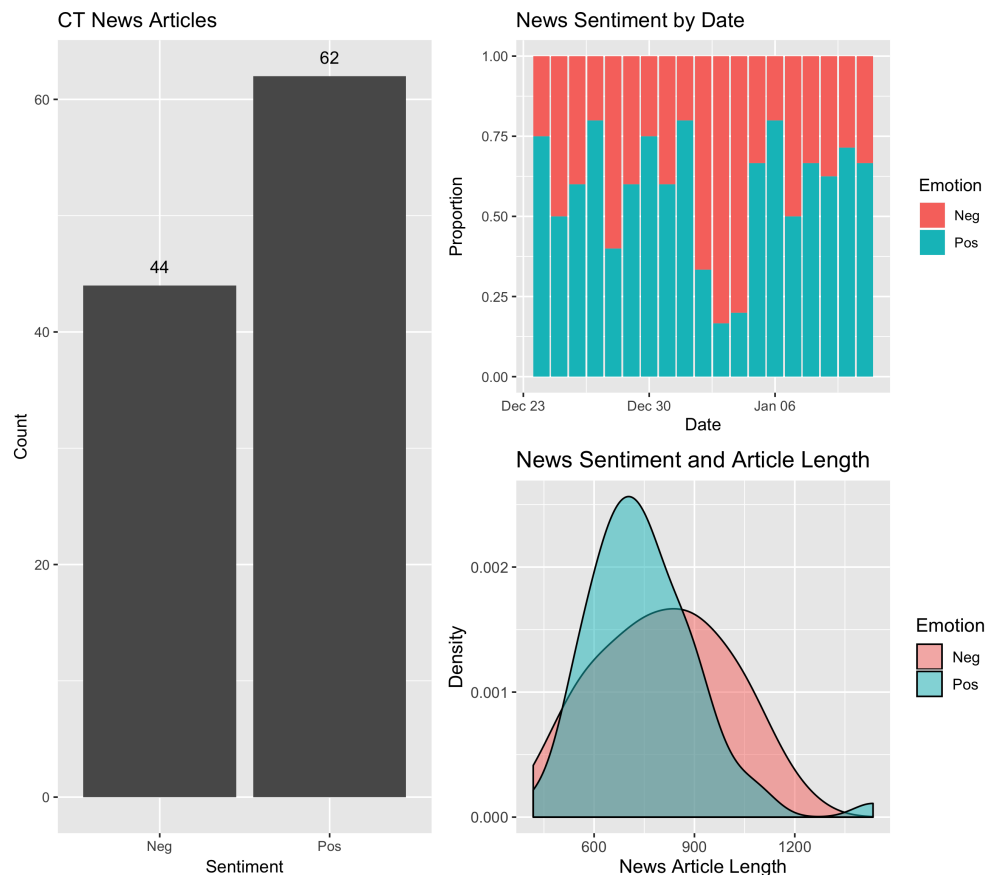
- Jan 5 假民調假消息毀了台灣的誠樸
 黑鷹墜毀人為因素待分析黑盒子初判動力正常沒亂流
 韓嘆蔡政府視軍公教如米蟲
 呂秀蓮批民進黨黨國威權已復辟
 美中東增兵警告伊朗勿開戰
 美伊若開戰殃及大陸一帶一路
 新年不快樂天災人禍危害全球
- Jan 4 林靜儀失言連扁都看不下去批太超過扯爆！蔡辦發言人主張統一是叛國
 川普發動無人機攻擊伊朗精神領袖誓言復仇革命衛隊指揮官遭斬首波斯灣戰雲密布
 陸反對動武以色列挺美
 台商痛批因不支持武統才盼和統
 消聲統派等同斬首兩岸和平
- Jan 3 黑鷹直升機失事8死5傷慟！參謀總長殉職
 去年1萬5000多人丟了飯碗勞工苦悶解僱及無薪假雙創新高
 港破獲非法無線電台逮5嫌
 中大重整校園耗費2.6億台幣
 記住67人讓台商台生回不了家
 陸配委屈愛台卻被當待宰羔羊
- Jan 2 韓競辦公布影片台版史諾登現形國安監控輿論入侵LINE私群
 看不下去吹哨者憤揭國家機器
 香港元旦大遊行與警爆衝突
 港府發聲明反擊捍衛法治
 包道格稱無需過度放大北京因素
 新年搶招財錢母9萬人擠爆紫南宮
- Jan 1 反滲透法民進黨鴨霸三讀！綠色恐怖人人自危
 101大樓幻彩300秒2020絢爛開場
 習近平細數中國夢未提台灣
 背景書架上父子新合照受矚目
 對港動之以情對台不再提親情
- Dec 31 實質與對岸脫鉤關上往來互動大門反滲透法兩岸禁止交流法
 誰下令保密30年中選會教育部說清楚國家機器遮掩論文門藍問蔡怕什麼
 香港商家自救黃藍經濟戰開打
 民陣元旦遊行已獲不反對通知
 敵視仇中陸無人敢幫台發聲
- Dec 30 選前強行闖關綠對選情沒信心！韓轟反滲透法人民脖子綁炸彈
 等一個人風雨澆不熄鋼鐵粉熱情！30萬韓粉喊凍蒜韓願為人民做牛做馬
 假特工王立強詐騙前老闆920萬
 美應恪守一中解決貿易分歧
- Dec 29 反滲透法對撞民意！反對聲浪從藍橘白蔓延到深綠獨派
 贊助2020台北晚會全球獨家轉播！愛奇藝直播跨年柯憂被法辦
 陸修改通過台商保護法鬆綁興利
 台商籲惠台政策入法更有保障

- 港再爆遊行衝突示威者嗆陸客回去
- Dec 28 英諷韓總機宋批反滲透法韓政見會嗆蔡被新系架空
「敵對勢力」、「滲透來源」定義朝野爭議大反滲透法協商無共識下周再戰
40年首次陸俄伊聯合軍演秀肌肉
以訟止戰美商界擬告華府
反滲透法若過關閹割兩岸條例
- Dec 27 負債比977%前高銀授信放水爆新事證慶富案劍指4大寇
反滲透法今協商藍促下會期新民意決定
反滲透法返威權百萬台人陷恐懼
台商憂見陸官員可能被誣入罪
- Dec 26 反滲透立法小英稱可討論綠黨團打臉31日要通過！韓辦怒轟蔡睜眼說瞎話
中市府重懲中火廢2許可證開罰900萬
陸辦奧亞運台選手享主場待遇！教育部提醒須遵守兩岸條例規範
波特王事件國台辦稱企業商業行為
- Dec 25 民進黨與南風合約曝光！藍爆暗黑網軍上線劍指民進黨中央
約詢審理馬英九案法官引發強烈反彈！監委當東廠藍發動下架陳師孟
陸日韓攜手共築新三國時代
陸穩住日韓台處境孤立
- Dec 24 三立及網軍黑韓無極限韓營今提告蔡吻童親民韓被黑嘸衛生
蔡賴謝菊等大咖都曾登陸竟批藍賣台小英曾說一中是唯一選擇
朝港放兩邊陸日韓經濟最優先
尊重內政文在寅對港疆表態
-

Section B. Text Analysis

I conduct a sentiment analysis of the news articles presented on the treatment website. Among all the 106 political news articles that study participants could read on the website, 62 of them attached more positive sentiment in coverage, and 44 of them attached more negative sentiment. We also find that positive articles are shorter in length than negative articles. More importantly, after a careful reading of each news article, I find that the majority of the positive articles deal with China-related issues (e.g., Belt-and-Road, One-Country-Two-Systems/cross-strait relations, Xi Jinping's 2020 New Year speech, China-Japan-South Korea free trade agreement, and restoration of Hong Kong's stability) and Han's political campaign. By contrast *The China Times* attached more negative sentiment in news coverage on issues of the incumbent Tsai and her political party, the Anti-Infiltration Act (a law regulating the influence of entities deemed foreign hostile forces on Taiwan's political processes such as elections), Hong Kong protesters, and conflicts between Iran and the United States.

Figure B.6: Sentiment Analysis



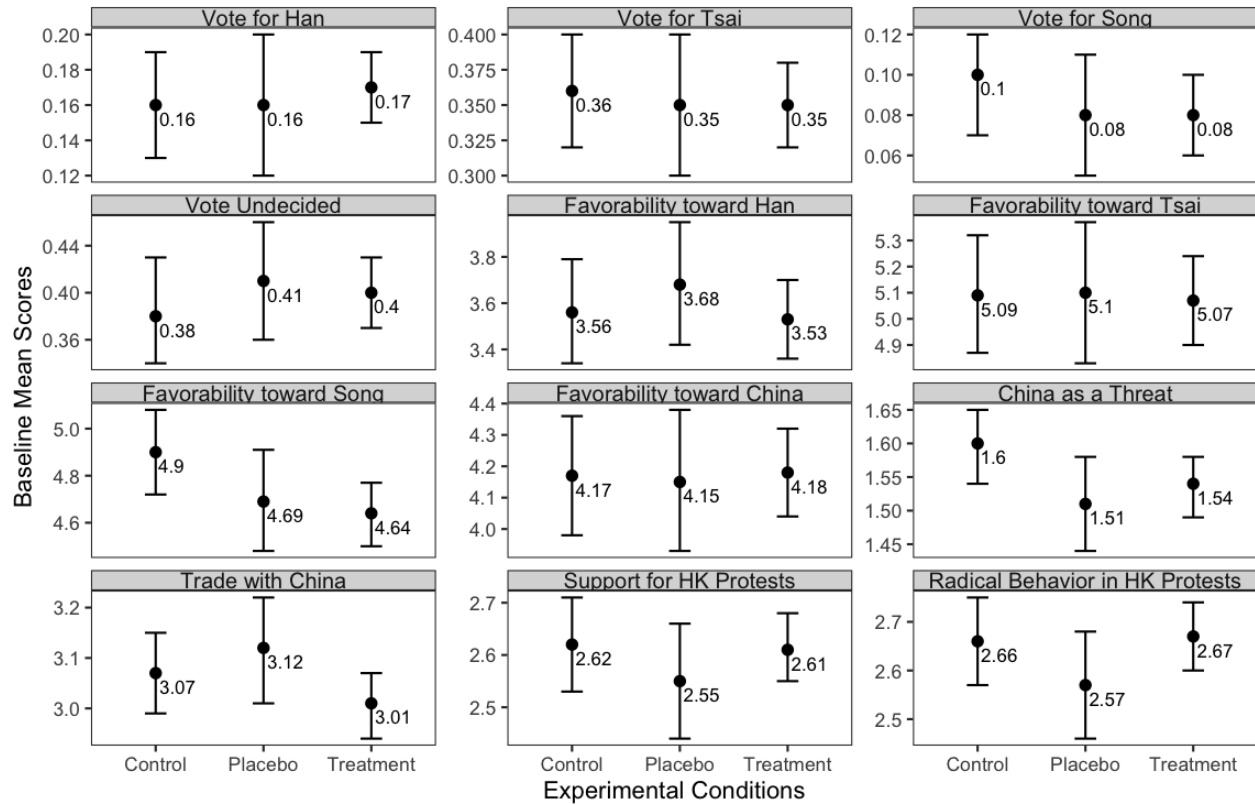
Section C: Descriptive Statistics, Baseline Outcome Scores by Experimental Conditions, and Sample Selection Bias

Table B.2: Summary Statistics

	N	Mean	SD	Min	Max
Panel A: All Participants					
<u>Vote for Han</u>					
Baseline	2,077	0.18	0.39	0	1
Endline	815	0.34	0.48	0	1
Change Score	815	0.12	0.43	-1	1
<u>Candidate Evaluation</u>					
Baseline	2,077	-1.32	4.42	-9	9
Endline	949	-1.19	5.06	-9	9
Change Score	949	0.04	3.39	-18	18
<u>Pro-China Index</u>					
Baseline	2,077	2.85	0.9	1	5
Endline	949	2.97	1.03	1	5.6
Change Score	949	0.11	0.84	-3.4	4.2
<u>Political Predispositions</u>					
Pan-Blue	2,077	0.31	0.46	0	1
Independent	2,077	0.4	0.49	0	1
Pan-Green	2,077	0.29	0.46	0	1
Panel B: Experimental Participant					
<u>Vote for Han</u>					
Baseline	1882	0.17	0.37	0	1
Endline	738	0.33	0.47	0	1
Change Score	738	0.14	0.43	-1	1
<u>Candidate Evaluation</u>					
Baseline	1882	-1.51	4.32	-9	9
Endline	861	-1.35	5.07	-9	9
Change Score	861	0.06	3.35	-12	18
<u>Pro-China Index</u>					
Baseline	1882	2.8	0.88	1	5.6
Endline	861	2.95	1.04	1	5.6
Change Score	861	0.12	0.85	-3.4	4.2
<u>Political Predisposition</u>					
Pan-Blue	1882	0.28	0.45	0	1
Independent	1882	0.42	0.49	0	1
Pan-Green	1882	0.3	0.46	0	1
Panel C: Nonexperimental Participants					
<u>Vote for Han</u>					
Baseline	195	0.34	0.47	0	1
Endline	77	0.45	0.5	0	1
Change Score	77	0.08	0.53	-1	1
<u>Candidate Evaluation</u>					
Baseline	195	0.57	4.9	-9	9
Endline	88	0.44	4.66	-9	9
Change Score	88	-0.1	3.76	-18	11
<u>Pro-China Index</u>					
Baseline	195	3.25	0.99	1	5.6
Endline	88	3.16	0.92	1	5.4
Change Score	88	0.05	0.73	-1.8	2.8
<u>Political Predisposition</u>					
Pan-Blue	195	0.52	0.5	0	1
Independent	195	0.23	0.42	0	1
Pan-Green	195	0.25	0.43	0	1

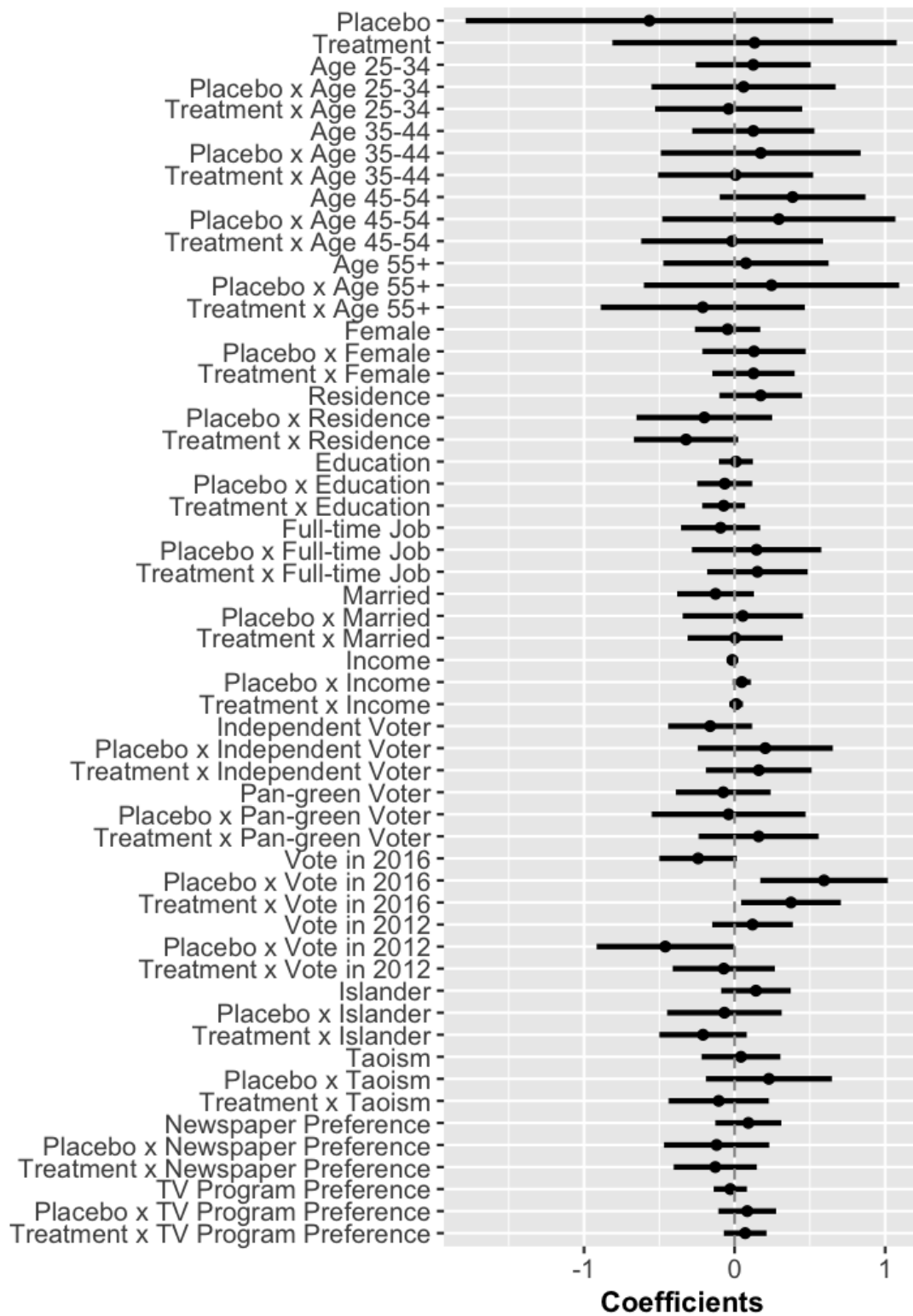
Note: This table reports descriptive statistics of outcome variables and political priors among all study participants (Panel A), experimental participants (Panel B), and nonexperimental participants (Panel C).

Figure B.7: Baseline Outcome Scores by Experimental Conditions



Note: The figure reports the mean outcome scores measured in the baseline survey (i.e., before the experiment) by experimental conditions. Estimates are drawn from the experimental sample ($N=1,882$). The bars indicate 95% CI. Results show that the scores are balanced across conditions, suggesting randomization was conducted properly.

Figure B.8: Analysis of Participation in the Endline Survey, Probit



Note: The figure reports the probit estimates of participation in the endline survey ($N=1,882$). All background variables (except assignment to treatment) are from the baseline survey. The bars indicate 95% CI.

Section D. Comparison of Control Group and Placebo Group

Table B.3: Comparison of Placebo Group and Control Group

	(1)	(2)	(3)	(4)	(5)	(6)
	Vote for Han	Vote for Tsai	Favor. Han	Favor. Tsai	China Index	Anti-Infi. Act
Placebo	0.0238 (0.250)	0.149 (1.330)	-0.0244 (-0.27)	0.00875 (0.090)	0.0318 (0.390)	0.102 (1.090)
Constant	-0.139* (-2.15)	0.0655 (0.910)	-0.0803 (-1.45)	0.0703 (1.230)	-0.146* (-3.20)	-0.176* (-3.03)
N	355	355	424	424	424	424

t statistics in parentheses + $p < 0.10$, * $p < 0.05$

Note: Comparison of standardized means in outcome scores, among participants in the control group, who were in the status quo between survey waves, and those in the placebo group, who received daily invitations to visit a news website containing real entertainment news from *The China Times*. The baseline group is the control group; the coefficients refer to (standardized) mean differences between the two groups.

Table B.4: Comparison of Placebo Group and Control Group on Pro-China Index

	(1)	(2)	(3)	(4)	(5)
	Favor. China	China Threat	Trade w/ China	HK Protest	Radical Behavior
Placebo	-0.028 (-0.34)	0.049 (0.520)	0.101 (1.020)	0.001 (0.010)	0.038 (0.390)
Constant	-0.081 (-1.53)	-0.107+ (-1.77)	-0.155* (-2.79)	-0.083 (-1.65)	-0.084 (-1.41)
N	424	424	424	424	424

t statistics in parentheses + $p < 0.10$, * $p < 0.05$

Note: Comparison of standardized means in five survey questions used to generate China Index between the control group and placebo group. The baseline group is the control group.

Section E. Pro-China Index Breakdown

Figure B.9: Regression Coefficients on China-Related Variables

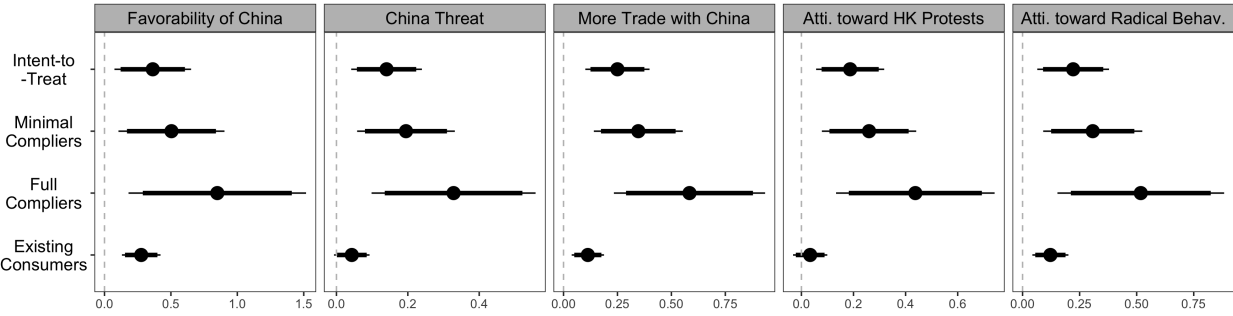


Table B.5: Treatment Effects on Pro-China Index, Breakdown

	(1)	(2)	(3)	(4)	(5)
	Favor. China	China Threat	Trade w/ China	HK Protest	Radical Behavior
Panel A: ITT					
Treatment	0.167* (2.470)	0.189** (2.780)	0.224** (3.290)	0.188** (2.810)	0.188** (2.760)
Constant	-0.0918* (-2.27)	-0.0879+ (-1.90)	-0.116* (-2.48)	-0.0829+ (-1.90)	(0.070) (-1.49)
Panel B: Two-stage Estimates					
Complier	0.671* (2.440)	0.761** (2.730)	0.901** (3.100)	0.758** (2.730)	0.757** (2.680)
Constant	-0.212** (-2.58)	-0.224* (-2.49)	-0.277** (-2.97)	-0.219* (-2.51)	-0.206* (-2.22)
N	861	861	861	861	861

t statistics in parentheses

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table reports standardized coefficients of ITT (panel A) and TOT (panel B) effects on questions about attitudes toward China-related issues. We use these variables to generate the China Index.

Section F. Vote Intent of Experimental Participants

Table B.6: Baseline Vote Intent among Experimental Participants

	Han	Tsai	Song	Undecided	Obs.
Pan-Blue	N=264 (49.40%)	N=48 (8.90%)	N=60 (11.24%)	N=162 (30.30%)	534
Pan-Green	N=5 (0.89%)	N=442 (78.50%)	N=21 (3.73%)	N=95 (16.90%)	563
Independent	N=42 (5.35%)	N=178 (22.68%)	N=77 (9.80%)	N=488 (62.10%)	785

Note: I asked all study participants in the baseline survey which candidate they would vote for if the 2020 Presidential Election were held today. The table is survey responses from those participants who are not existing consumers of *The China Times* and thus are in the experimental sample (N=1882).

Table B.7: Baseline Vote Intent among Experimental Participants Completing the Endline

	Han	Tsai	Song	Undecided	Obs.
Pan-Blue	N=133 (52.99%)	N=21 (8.37%)	N=28 (11.16%)	N=69 (27.49%)	251
Pan-Green	N=1 (0.39%)	N=208 (80.62%)	N=9 (3.49%)	N=40 (15.50%)	258
Independent	N=22 (6.25%)	N=84 (23.86%)	N=38 (10.80%)	N=208 (59.09%)	352

Note: I asked all study participants in the baseline survey which candidate they would vote for if the 2020 Presidential Election were held today. The table shows survey responses from those participants who are in the experimental sample and have completed both baseline and endline surveys (N=861).

Table B.8: How Those Voting Han Changed from Vote Intent to Realized Choice (descriptive statistics by conditions)

	From Tsai	From Soong	From Undecided	No Change	Obs.
Control	5 (0.096%)	4 (0.076%)	17 (0.325%)	26 (0.50%)	52
Placebo	1 (0.023%)	2 (0.047%)	15 (0.357%)	24 (0.571%)	42
Treatment	11 (0.072%)	12 (0.078%)	63 (0.414%)	66 (0.434%)	152

Note: The column indicates how participants who voted for Han in election moved from their vote intent measured in the baseline survey. Four possible scenarios are: (1) Tsai to Han: participants initially intended to vote for Tsai Ing-wen before the experiment and selected Han Kuo-yu in the election. (2) Soong to Han: participants initially intended to vote for James Soong and selected Han Kuo-yu in the election. I view this change as partial conversion. (3) Undecided to Han: participants were initially undecided in the baseline and voted for Han Kuo-yu in the election. (4) No change: participants intended to vote Han Kuo-yu in the baseline and selected him in the election. This table provides information about how the treatment moved the vote. It shows that the effects occur mainly by persuading undecided voters rather than converting voters away from their initial vote intention.

Table B.9: Conversion, Activation, and Reinforcement among Partisan Participants

	Conversion Away	Partial Conversion	Conversion Home (Tsai/Soong)	Activation	No Change	Obs.
Control	1 (0.023%)	0 (0%)	3/3 (0.142%)	11 (0.261%)	24 (0.571%)	42
Placebo	1 (0.035%)	0 (0%)	0/1 (0.035%)	6 (0.214%)	20 (0.714%)	28
Treatment	0 (0%)	2 (0.02%)	6/10 (0.161%)	21 (0.212%)	60 (0.606%)	99

Note: Among pan-Blue and pan-Green participants who choose Han in the election. Conversion Away refers to pan-Green participants who initially intend to choose Tsai ultimately vote for Han. Partial Conversion refers to undecided pan-Green participants vote for Han in the election. Conversion Home refers to pan-Blue participants who initially intend to choose (Tsai/Soong) ultimately vote for Han. Activation refers to undecided pan-Blue participants vote for Han in the election. No Change (or reinforcement) refers to pan-Blue (pan-Green) participants who intend to choose Han vote for Han. N=169

Table B.10: How Independent Participants who Selected Han Changed from Vote Intent to Realized Choice (descriptive statistics by conditions)

	From Tsai	From Soong	From Undecided	No Change	Obs.
Control	1 (0.10%)	1 (0.10%)	6 (0.60%)	2 (0.20%)	10
Placebo	0 (0%)	1 (0.07%)	9 (0.64%)	4 (0.28%)	14
Treatment	5 (0.09%)	2 (0.04%)	40 (0.75%)	6 (0.11%)	53

Appendix C

Supplementary Information for "Persuasive Backfiring"

Survey Questionnaire for Key Variables

I. Manipulation Check

- In general, do you think to what extent local congress delegates are willing to respond ordinary people's interests?

II. Outcome Variables

- Everyone who cheats on their taxes should be held accountable (*Accountable*)
- People should just have to pay what they feel is a fair amount of taxes to governments (*Reasonable*)
- Which of these expressions you agree with the most (*Acceptable*)
 - Tax fraud is unacceptable under any circumstance. It is a matter of principle and fairness.
 - Fraud and taxes are inseparable, everyone evades taxes to some extent, and this is how the system is sustainable.

III. Moderators

- Political Sophistication
 - Who is not the current members of the Politburo Standing Committee?
 - Other than CCP, how many political parties are there in China?
 - When did the central government implement tax-sharing reform?
 - Where is the headquarter of the Shanghai Cooperation Organization?
 - Who is the current chairman of the standing committee of the National People's Congress?
 - Who is the current Prime Minister of the United Kingdom?

- Self-Monitoring (The first three items refer to positive-keyed items and the following three are negative-keyed items)
 - I put on a show to impress or entertain others
 - I would probably make a good actor
 - In a group of people, I am rarely the center of attention
 - At a party, I let others keep the jokes and stories going
 - I can make speeches on the spot even on topics on which I have almost no information
 - I find it hard to imitate the behavior of other people

Table C.1: Treatment Effect on Tax Compliance Attitudes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Tax Morale	Accountable	Reasonable	Acceptable	PCA	Tax Morale	Accountable	Reasonable	Acceptable	PCA
Treatment	-0.208*** (0.000)	-0.254** (0.005)	-0.267* (0.016)	-0.104 (0.158)	-0.301** (0.002)	-0.228*** (0.000)	-0.328*** (0.001)	-0.241* (0.037)	-0.114 (0.279)	-0.347** (0.002)
Female						0.230** (0.008)	0.458** (0.005)	0.0960+ (0.099)	0.137 (0.437)	0.406* (0.011)
Age						0.00290 (0.487)	0.00591 (0.613)	-0.0127 (0.477)	0.0155* (0.034)	0.0120 (0.105)
CCP						0.00286 (0.954)	-0.123 (0.469)	-0.0911 (0.640)	0.223+ (0.084)	0.0687 (0.203)
Income						-0.0126 (0.579)	-0.0551 (0.343)	0.0449* (0.044)	-0.0276 (0.313)	-0.0428 (0.400)
Constant	0.108* (0.016)	0.132+ (0.081)	0.139* (0.026)	0.0541 (0.343)	0.157+ (0.059)	-0.00549 (0.984)	0.161 (0.623)	0.201 (0.689)	-0.378 (0.182)	-0.134 (0.742)
Obs.	119	119	119	119	119	119	119	119	119	119

p-values in parentheses+ $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$

Note: This table reports the standardized coefficients of the treatment effects on the outcomes of interest. The first five models are bivariate models regressing respondents' outcome scores on their treatment condition. The following five models are multivariate models regressing outcome scores on treatment condition and background characteristics.

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